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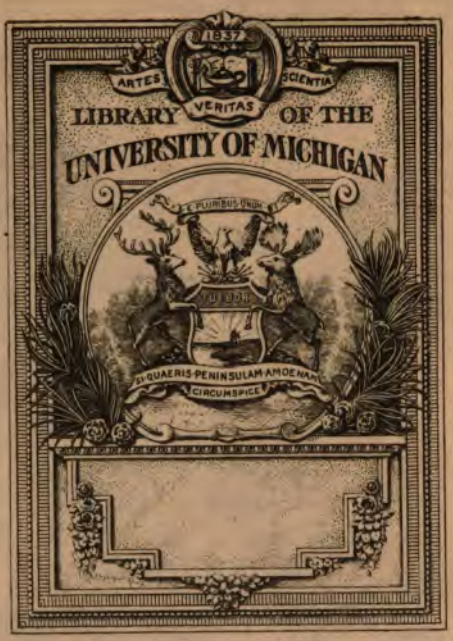
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THE President and Council, convinced of the advantages resulting from the early publication of the Papers with which the Medical and Chirurgical Society may be favored, and which may be deemed worthy of being laid before the Public, have lost no time in editing this Third Volume of the Society's Transactions. The utility of the plan of early publication, receives an important confirmation from the experience of the Royal Society, and some other Scientific Bodies in this country; and though the President and Council by no means wish to pledge themselves to the precise periods at which future volumes may appear, (which must necessarily depend on the number, as well as the importance of communications made to the Society,) yet they have every reason to expect, that they will be able to publish a part of a Volume, if not a whole one, at the end of every season.

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CONTENTS.

Page

I. Facts and Observations respecting Intermittent Fevers, and the exhalations which occasion them. By Sir Gilbert Blane, Bart. M.D. F.R.S. Physician in Ordinary to the Prince Regent 1

Appendix, containing remarks on the Comparative Health and Population of England and Wales, at different periods 34

II. History of a remarkable case of Ovarian Dropsy. By Thomas Chevalier, Esq. F.L.S. Surgeon Extraordinary to the Prince Regent, and Surgeon to the Westminster General Dispensary 41

III. A Case of difficult parturition, occasioned by a Dropsical Ovarium forming a Tumor in the lower part of the Pelvis. By Samuel Merriman, M.D. Physician Accoucheur to the Middlesex Hospital, and the Westminster General Dispensary 47

IV. A Case of diseased Testicle, accompanied with disease of the Lungs and Brain, and terminating fatally. By Henry Earle, Esq. Communicated by William Lawrence, Esq. To which is added a Note by Mr. Lawrence, containing some particulars of the histories and dissections of four cases 59

V. Description of an improved method of tying diseased Tonsils. By Thomas Chevalier, Esq., F.L.S. Surgeon Extraordinary to the Prince Regent, and Surgeon to the Westminster General Dispensary 80

VI. Cases of Cynanche Laryngea. By J. R. Farre, M.D. 84

Page

VII. History of a case of Anæsthesia. By John Yelloly, M.D. Physician to the London Hospital . . . 90

VIII. Account of a case of spontaneous Extravasation within the Theca Vertebralis, which soon terminated fatally. By Thomas Chevalier, Esq. F.L.S. Surgeon Extraordinary to the Prince Regent, and Surgeon to the Westminster General Dispensary 102

IX. Observations on Disbetes Insipidus. By John Bostock, M.D. of Liverpool 107

X. Cases of premature Labour artificially induced in Women with distorted Pelvis; to which are subjoined some observations on this method of practice. By Samuel Merriman, M. D. Physician-Accoucheur to the Middlesex Hospital, and to the Westminster General Dispensary 123

XI. Experiments on the Bark of the Coccoloba Uvifera. By John Bostock, M.D. of Liverpool . . . 146

XII. A Case of Splenitis, with further remarks on that disease. By Robert Bree, M.D. F.R.S. . . . 155

XIII. Account of the Muscles of the Ureters, and their effects in the irritable states of the Bladder. By Charles Bell, Esq. F.R.S. Ed., Teacher of Anatomy in Great Windmill-street. Communicated by Dr. Curry . 171

XIV. History of a case in which a Calculus was voided from a Tumor in the Groin. By T. Copeland, Esq. Assistant Surgeon to the Westminster General Dispensary 191

XV. General Views of the Composition of Animal Fluids. By J. Berzelius, M.D. Professor of Chemistry in the College of Medicine, at Stockholm. Communicated by Dr. Marcet 198

XVI. A Case of Fungus Hæmatodes. By George Langstaff, Esq. Surgeon, New Basinghall-street. Communicated by William Lawrence, Esq. . . . 277

Page

XVII. History of a severe affection of the Organs of Respiration; with the appearances on dissection, and remarks. By A. P. Wilson Philip, M.D. Physician to the Infirmary at Worcester. Communicated by Dr. Baillie 290

XVIII. An Account of a new mode of treatment in Chronic Rheumatism, and especially in Sciatica. Communicated by Alexander Marcet, M.D. F.R.S. one of the Physicians to Guy's Hospital . . . 310

XIX. Appendix to the Paper on Cynanche Laryngea, containing remarks on the distinction between that disease and Cynanche Trachealis. By J. R. Farre, M.D. 323

XX. Some Remarks on the Use of Nitrat of Silver, for the detection of minute portions of Arsenic. By Alexander Marcet, M.D. F.R.S. one of the Physicians to Guy's Hospital 342

XXI. History of a case of remitting Ophthalmia, and its successful treatment by Opium. By James Curry, M.D. F.A.S. &c. and Senior Physician to Guy's Hospital 348

List of Donations to the Society . . . 373

Index 375

Explanation of the Plates . . . 381

FACTS AND OBSERVATIONS

RESPECTING

INTERMITTENT FEVERS,

AND THE

EXHALATIONS WHICH OCCASION THEM.

By Sir GILBERT BLANE, Bart. M.D. F.R.S.
PHYSICIAN IN ORDINARY TO THE PRINCE REGENT.

Read March 3d, 1812.

HAVING been sent by the Government of this country on a special mission to the Island of Walcheren, in the autumn of the year 1809, in order to ascertain the nature and causes of the great sickness and mortality prevailing in the British army in Zealand, and to make a report of my enquiries: having been also sent by the Admiralty to Northfleet, in the autumn of 1810, in order to investigate the nature and situation of that spot in point of health, with a view to decide, whether any objection in point of unhealthfulness would arise to the formation of a projected dock-yard, and other naval establishments at that place, some observations have occurred to me in executing these duties, which appeared

to me sufficiently interesting to be laid before this Society.

During my residence in Walcheren, I not only visited all the hospitals, but inspected, with the permission of the Commander in Chief, the whole returns of the army from the time of their disembarkation, in order to ascertain the progress and extent of the sickness and mortality. The result of these enquiries is what I now propose to communicate, and, in describing the nature, and detailing the ravages of the prevailing disorder, I shall borrow the greater part of what I have to say, from my official communications.

I arrived in the island on the 30th of September, and remained till the 13th of October following. During my stay I stated to the government, that I found so great a proportion of the sick to consist of those affected with the intermitting and remitting fevers peculiar to marshy countries, that there could be no doubt that the sickness of the army was owing to that cause.

The fever commonly called typhus, with which armies in ordinary circumstances are chiefly affected, had been rare, and dysentery, which, in the history of former campaigns* in the Low Countries,

* See Sir J. Pringle's Work on the diseases of the army, in which there is a most accurate history of the diseases prevailing in the British armies in Zealand and other parts of the Low Countries.

proved so severe a scourge to our armies in the autumnal months, had been as yet but little felt. Both these diseases, however, had begun to shew themselves at Flushing, where the accommodations were at best far inferior to those at Middleburgh; but were then still more so, in consequence of most of the buildings having been injured by the shot and shells thrown into the town during the siege. In the large and elegant city of Middleburgh, the accommodations were excellent, as we had not only the advantage of the hospitals formerly belonging to the Dutch and French troops, but the spacious and airy warehouses of the Dutch East India Company: this having been formerly the great emporium of Indian commerce. Here I found no typhus nor dysentery, but the prevalence of these two diseases was very remarkable at Flushing, particularly in one regiment, of which all the medical officers were either absent or dead, and of which the sick originally affected with the endemic disease were suffering also from typhus and dysentery, in consequence of the want of cleanliness as well as of proper medicines, diet and attendance. This fact affords a proof of the necessity of general hospitals on actual service, as well to prevent the generation and extension of infection, as to afford relief to the regimental establishments, when the sick and wounded accumulate beyond their means of accommodation. It is evident in the present instance, how necessary this is even in stationary service; but with regard to the ordinary service of a campaign, where armies

are in motion, and where regimental surgeons and their assistants must be present with the regiments, general hospitals may be regarded as absolutely indispensable. There ought also to be a liberal establishment of medical officers attached to such hospitals in case of emergencies of service, and to fill up such casual vacancies as may occur in regiments.

I found myself under the painful necessity, therefore, of stating, that the sickness on this island did not diminish. It appeared from the latest general weekly return, that there were two thirds of the whole numeral strength of the army incapable of duty. The mortality during the last four weeks had been about 1000. All the battalions were affected nearly in an equal degree; and it does not appear, that their illness was connected with the nature of their duty, or that it was owing to privations or neglect of any kind; for those were equally sickly, who had enjoyed the utmost ease and comfort in cantonments, as those who had been engaged in the siege of Flushing.

Nor was this great sickness imputable to any thing unfavourable in the weather at this season, in comparison of former years. On the contrary, the native inhabitants affirmed, that they were then less sickly than usual at the same season of the year, and they accounted for this, from the uncommon quantity of rain that had fallen the last two months: for they

consider it as fully established by observation, that the most sickly years are those, in which there had been great drought and heat in the latter end of summer and the early part of autumn; owing, probably, to the increased exhalation, and the more concentrated foulness of the stagnant water produced by these causes.

I found upon enquiry, that a like degree of sickness prevailed among the French troops who occupied Flushing during the last seven years; and that in former times, the Dutch troops, from the northern parts of the United Provinces, suffered equally. As the army had not suffered either from the scantiness and bad quality of provisions, nor from want of proper accommodation, nor from hardships and fatigue, it admits of no doubt, that the unfortunate state of the army here, was solely imputable to the contamination of the air from a soil the most productive of deleterious exhalations of any perhaps in Europe, producing an endemic fever which has at all times been particularly severe upon strangers in the autumnal months. I find also upon enquiry, that though this is by far the most sickly season, the residents of this and the neighbouring islands do not enjoy, at any season, the same degree of health, as the inhabitants of the more salubrious parts of Europe.

From this statement, it will be clearly perceived, how much the causes of sickness were out of the

reach of human control. There were two facts, however, which afforded some encouragement for the employment of artificial means, in counteracting the overpowering influence of natural causes. One was, that those belonging to the upper orders of society in Walcheren were always less affected with its endemic fevers than the poorer inhabitants: the other, that the British officers suffered less in this campaign, than the private men, as will be seen by an abstract of the returns. As this latter must be owing to some circumstances of superior accommodation and diet, there was encouragement to attempt some improvement in these respects, though the situation of the common soldier was as comfortable as belongs to his condition. With this view I suggested the use of stoves in the barracks as well as the hospitals, in order to promote the dryness, warmth, and purity of the air. I also represented, that considerable benefit might arise from the men being supplied with a hot breakfast. It may likewise be remarked, that those who slept in the upper stories of houses, were less liable to the disease, and had it in a milder form, than those who slept on the ground floors. The testimony of the natives is in favor of this observation. We had a striking confirmation of it in the visit we paid to the party accommodated at Fort Rammekins. To the observation of General Monnet (the French general who commanded during the siege) with respect to the good effect of a small quantity of ardent spirits in the morning, I may add a recom-

mendation of mixing pepper freely with the broth and other articles of food.

There were, in the beginning of October, when I arrived, considerably more than one half of the army sick, or convalescent in hospitals. This amount was not owing merely to the numbers accruing from those who were daily taken ill, but was swelled in consequence of the small number of discharges, and the numbers of convalescents waiting for a passage to England; for under the influence of the endemial air, recoveries were slow and imperfect, and relapses very frequent, not only among the few who were discharged, but among the convalescents at the hospital, some of whom, when apparently in a fair way of doing well, would unaccountably drop down dead. This made me urge the conveyance of such subjects to England, with as little delay as possible. There were then 6000 subjects proper for being transported to England; and I recommended, in conjunction with Dr. Macgregor, the superintendant of the military hospitals, that line of battle ships, with their lower deck guns taken out, should be sent from England for this purpose, there being at Walcheren only the means of conveyance for 1000 men. This measure was rendered further necessary, by the rapid accumulation of sick in the hospitals, some of which were already over-crowded, and, if not relieved, must in themselves have proved a source of additional sickness and infection.

One of the most important circumstances in the operation of marsh miasma on the human body is the power of habit in mitigating its influence. The natives are not a robust people; they are of a very wan and sickly hue, and have all suffered more or less from the bad air which they breathe. The children of both sexes are very subject to glandular and abdominal complaints; and the adults, particularly those of the lower orders, have all of them, some time or other in the course of their lives, laboured under the endemic intermittent. They are, however, infinitely less subject to intermittent fevers than strangers. It was curious to remark, in conversing with the natives, even persons of education, and medical practitioners, that they would not admit their country to be more unhealthy than any other; and when they were asked to account for the great sickness prevailing among our troops, they mentioned some frivolous circumstance in diet and habits of life, but would allow nothing to be ascribed to the insalubrity of the air. However unfounded this prejudice may be, it is strongly expressive of the great difference in point of health between natives and strangers. These strangers are also variously affected according to the district from which they come. It was found, that of the British troops, the natives of mountainous countries, and dry soils, were more frequently affected than the natives of flat and moist districts. It is also well ascertained, that strangers, if they survive the first attacks, become thereafter much less lia-

ble to the endemic intermittents. This was well proved and illustrated in a *Memoire* which was discovered in the house of the French general Monnet *.

It was there recommended that troops should not be frequently changed; for when it was the custom to send battalions from Bergen op Zoom, every fourth night, in succession, to work on the lines of Flushing, these men never failed, on their return, to be taken ill in great numbers. General Monnet therefore advised, however displeasing it might be to Officers, that a stationary garrison should be retained in Walcheren, in order that it might be habituated to the air, *(acclimaté)* and he instanced a French regiment, which suffered in the second year of its being stationed there only one half the sickness and mortality which it suffered the first year, and hardly suffered at all the third. There were some other important remarks in the *Memoire*, such as, that when it might be necessary to reinforce the garrison, this should be done early in winter, in order that the men might be habituated to the climate before the return of the sickly months, which he reckoned to be June, July, August and September. He also recommended, that men who mount guard or who are employed in any

* General Monnet was an able man, and had commanded at Flushing during the whole seven years in which the French had possession of that place. We may therefore admit the authority of his judgment and experience.

other duty exposing them to cold damp or fatigue, should have a double ration of spirits (genievre,) and that that there should be an additional allowance of this, and also of vinegar during the sickly months. Another remark of this General was very consolatory to us at this time, namely, that the oldest inhabitant did not remember a year, in which this endemic had not disappeared before the end of October.

The expedition to Zealand sailed from the Downs on the 28th of July, and made good their landing on Walcheren, and North and South Beveland, on the 31st of July and the 1st of August. The only military operation of consequence was the siege of Flushing, which was invested on the 1st of August, and capitulated on the 15th of the same month. In the beginning of September, the islands of North and South Beveland were evacuated, and that part of the army which occupied them, returned to England; about 18000 being left to garrison Walcheren. More than one half of these died, or were sent to England on account of sickness in the course of the three following months; and the island was finally evacuated on the 23rd of December of that year.

The following Tables exhibit a view of the course of the sickness and mortality. I was enabled to bring them down to the end of the campaign, the Commander in Chief having obligingly allowed me to extract from the returns deposited at the War

Office, what was wanting in the notes which I had taken in Zealand.

I am also enabled to state, on the authority of Dr. Bancroft*, and Mr. Keate the surgeon-general, that the whole number of sick sent to hospitals in Zealand, between the 21st of August and the 18th of November, 1809, amounted to 26846, including relapses, and that the number of sick, including a small number of wounded conveyed from thence to England, between the 21st of August and the 16th of December, amounted to 12863; and that many instances occurred in those who returned to England apparently in health, in whom the endemial disease of Zealand appeared after the slight fatigue of a march.

Account of the Sickness and Mortality of the Army, in the Islands of Zealand, abstracted from the Monthly Returns, 25th August, 1809, and the three subsequent Months.

HEAD QUARTERS, FORT BATHZ, SOUTH BEVELAND.

DATE of the Monthly Return.	TOTAL.		SICK.			DIED.	
	Rank and File and Non-Commissioned Officers.	Commissioned Officers.	In Quarters.	In Hospitals.	Total.	Rank and File and Non-Commissioned Officers.	Officers.
25 Aug. 1809.	41642	1879	988	1713	2701	† 114	‡ 7

* Essay on the Yellow Fever, page 303.

† In this number 100 who were killed and died of wounds are included, so that only 14 died of disease.

‡ Of these one was killed as above, and 6 died of wounds, so that none died of disease.

HEAD QUARTERS, MIDDLEBURGH, IN WALCHEREN.

DATE of the Monthly Returns.	TOTAL.		SICK.			DIED.	
	Rank and File and Non-Commissioned Officers.	Commissioned Officers.	In Quarters.	In Hospitals.	Total.	Rank and File and Non-Commissioned Officers.	Officers.
25 Sep. 1809.	16931	723	3829	5000	8829	883	29
25 Oct.	11921	611	2845	3027	5872	760	9
25 Nov.	6297	452	469	624	1093	196	3

Account of the Sickness and Mortality in the Island of Walcheren, abstracted from the Weekly Returns, dated the 10th September, and the twelve subsequent Weeks.

DATE of the Weekly Return.	Rank and File and Non-Commissioned Officers.			Officers.		
	Total.	Sick	Died.	Total.	Sick.	Died.
10 September	17870	6931	221	770	No ret.	4
17	17410	8141	277	765	235	7
24	16409	8754	287	782	191	3
1 October	16156	9127	254	748	172	2
8	15276	8969	217	719	168	3
16	——	——	Return	mislaid	—	
23	13017	7145	128	655	113	
31	11747	6228	121	292	80	1
7 November	——	——	Return	mislaid	—	
14	8868	3799	40	559	45	
21	7926	1226	36	543	47	
29	6261	1158	30	383	30	

The first circumstance which strikes the eye on inspecting these Tables, is the smallness of the mortality in the first month of service. It not only

proves, that several weeks are necessary for these deleterious exhalations to act upon the system, so as to produce disease, but that the rate of this mortality is so much less than in ordinary circumstances, as to stagger one's belief. According to the statement here exhibited, only 14 died of disease in 41462 in a calendar month, and not one officer. I at first distrusted my own accuracy in making the abstract; but repeated examination convinced me that I was correct. I next distrusted the accuracy of the returns, but the adjutant-general assured me that there was not the smallest reason to suspect an error. According to the population returns of 1801, the smallest degree of mortality in any of the counties of England and Wales, was in Pembrokeshire; and was 1 in 76. The greatest mortality was in London; and was 1 in 31. But it is not fair to compare the mortality of an army, with that of the general population; for the latter includes all ages, sexes, and constitutions, whereas the former consists of the robust part of the male sex, in the prime of life. The computation being made on the like class, it would appear, that there is a considerably smaller rate of mortality than in people of the same age in England; for according to Simpson's tables, the mortality here in men and women from 20 to 45 is at the rate of one in 50 annually; but the annual rate of it in this army, if computed by the month ending the 26th of August, would be only 1 in 248.

This low rate of mortality will appear in a still more striking point of view, when compared with that of fleets and armies elsewhere: for at Coxheath in the year 1779*, the annual mortality was at the rate of 1 in 109; and during the time in which I kept records of the fleet in the West Indies, the lowest in any month, April 1782, was equal to an annual mortality of 1 in 72; and the actual annual mortality about that time in the army was 1 in 4, and in some particular spots more than one half.

Was this small rate of mortality in Zealand at this period, owing to the superior health and strength of those men who compose an army above the persons of both sexes, on whom the calculation is made in civil society? Or is it owing to this, that in the beginning of an expedition men's minds are in that elated state, from the sanguine hopes of victory and success, which is favorable to health? I have remarked elsewhere †, that in fleets, impressions of this kind have a striking effect on the health of men. Probably both the abovementioned circumstances had a share in keeping down the mortality at this period. This however was of short duration; for it will be seen by the Table, that the sick had begun to increase rapidly at this time; and that before the 10th of September, the mortality had become very great. As there is no

* See Blane's Observations on the Diseases of Seamen, page 170. Third Edition.

† Ibid. page 77.

account in these Tables, of the numbers taken ill in the intermediate times, nor of those discharged cured; nor any accurate statement of the number sent to England from time to time, we have no scale of the decreasing sickness, as the season changed; except the weekly diminution of the number of effective men. Thus it will appear by a calculation made on Table 2nd, that in the week between the 17th and 24th of September, the effective force was reduced from 9269 to 7655, so that 1 in 5.7 that is, about 2 in 11, had in that time become unfit for duty. It will appear by a like calculation, that on the 23rd of October, the effective force was reduced to 5872; and that in the course of the succeeding week, it was reduced to 5519; so that 353 had in that time become unfit for duty, that is, 1 in 1.094, or about 1 in 11. By this rule, the sickness was diminished by one half in the course of a few weeks, making allowance for the greater number that will be taken ill at the beginning of a campaign: for the most susceptible will necessarily first fall ill. No fair judgment can be formed from the returns of November, for reinforcements, of the amount of which I am uninformed, arrived from England in the course of that month to assist in the evacuation of the island. From all the evidence however, I could procure, the number of seizures continued to diminish as the winter advanced, conformably to what we had been told by the natives.

The Island of Walcheren is 13 miles long from east to west, and 9 miles from north to south. The capital of this island and of all Zealand is Middleburgh, an open town in the centre of the island, but communicating with the sea by a broad and deep canal, continued from a natural navigable inlet, leading to Rammekens, on the south-east quarter of the island. Flushing on the south side of the island is the principal sea-port and arsenal, and the only place of strength. The whole island, with the exception of some hills, or rather mounds of sand on the western shore, is a dead flat, below the level of the sea at high water and preserved from inundation by dykes. The soil consists of a fine white sand known in the eastern counties of England by the name of silt, and about a third part of clay. It is divided into small square inclosures, by ditches, which serve as drains; and these were about two thirds full of turbid water when I was there. They emit no smell, that I could perceive; but I was sensible of a bad smell proceeding from some ponds of stagnating water. The soil seems to be a mass of alluvial matter like the deltas of great rivers; and the whole islands of Zealand seem to have been formed by the *detritus* carried down by the Rhine and Scheldt, and forming accumulations for a long series of ages. There is a poison in the exhalations from such soils, the nature of which is entirely unknown. It is not animal putrefaction; for it is perfectly well ascertained, that those who are exposed to putrid vapors, such

as anatomists and tanners, are not affected by complaints of this kind; nor indeed by any complaint, unless these vapors are very concentrated; and the disease in that case is not an intermittent fever. Water in a state of stagnation, without any ascertainable principle of contamination, seems to generate these exhalations. It is only from the absence of stagnation, that we can account for the Delta of the Nile not producing the same disease as Zealand. This is so far from being the case, that Lower Egypt is one of the most healthful countries in the world, and is not infested with endemic intermittents. This remark did not escape the geographer* Strabo; and he assigns as the cause that the stagnation of the water was prevented by the annual inundation of the Nile. It appears also, from a work of Dr. Macgregor, that intermittent fevers, though not unknown, are not endemic in Egypt†. On the other hand we know from the medical history of Minorca‡, that, though this island consists of a rocky bottom, and very thin soil, yet in consequence of some stagnant water in channels and pools, severe intermittents are very common. I need hardly mention, that the plague is no exception, this being a disease depending on *human* efflu-

* Vid. Strabon. Geograph. lib. 17, page 1143, Amstelodam, 1707.

† Vid. Medical Sketches of the Expedition to Ægypt from India, by James Macgregor, M. D., page 99 and 163. London 1804.

‡ Vid. Cleghorn on the Diseases of Minorca.

via, and entirely unconnected with the nature of the soil.

An intelligent general officer on the expedition to Egypt in the year 1800, who had served in all climates, assured me, that he had nowhere seen so little sickness and mortality from disease; for sickness, even including the plague, was less destructive than in any other country in which he had served; so that there was here an exception to a rule which holds everywhere else, that disease is more fatal than the sword; for more were killed, or died by wounds, than by sickness, including even those who died of the plague. Nor can it be alleged, that humidity alone may have the effect of producing intermittent fevers; for the vapor of pure fresh water, when not in a state of long stagnation, is found to be free from any bad effects upon the greater number of constitutions. It is remarkable, that though much greater quantities of rain fall in the western parts of England than the eastern, the average in some counties of the former being more than double of what it is in those of the latter, yet it does not appear that health is in the least affected by this circumstance; and seamen, even in the thickest fogs on the banks of Newfoundland, for many days together, preserve their health perfectly. This poison, therefore, is some principle, with the nature of which we are still unacquainted. There are also certain species of decayed organic matter, the exhalations from which are not at all

productive of agues nor any other disorder. I allude to bogs or peat mosses. This is fully proved in Scotland, but still more in Ireland, where there are immense tracts of this soil, without any hurtful influence upon health. It might naturally be expected also, that the swamps round Venice would be productive of endemic fevers. This is not the case; and it is probably owing to the water which forms them, being sea-water.

The *miasmata* in Zealand, are more noxious than the like exhalations in England; the intermittents in the former, being more violent, untractable, and fatal, than those which occur in the fenny counties, in the eastern parts of our own country. I estimate this violence, by the high degree of febrile heat and delirium, by the excessive secretion of bile, the want of distinct intermissions, and the more frequent swellings of the liver and spleen, these taking place in the course of a very few weeks, which in England seldom occur but under a long continuance, or from frequent relapses of the disease.

The exhalations of the soil in tropical climates, extend farther, and are still more malignant than those of Zealand. Ships at the distance of 3000 feet from swampy shores, (a distance to which it did not extend in Zealand,) and even farther, were affected by the noxious exhalations, according to my own observations and those of others in the

West Indies; and I have been credibly informed of the like fact, with regard to the India ships in the channel which leads to Calcutta. This greater density and malignity of the exhalations, might naturally be expected from the greater intensity of atmospheric heat.

A medical gentleman belonging to the army in St. Lucia, one of the Caribbee Islands, in the year 1781, at which time I was Physician to the fleet on that station, favored me with the following statement, which throws considerable light on the subject here treated of.

" The Fevers in general are of the low kind ter-
" minating in intermittents.

" Unhealthy situations are the causes of many
" diseases here, particularly the worst sort of fever
" and intermittents.

" One regiment, viz. the 90th on the Morne
" Fortunée lost 271 men; the 91st on the side of
" the hill 318; the 89th in *Grand Cul de Sac* at the
" the bottom 486.

" The hill or morne is above the level of the sea
" 872 feet."

STATE OF SICKNESS AND MORTALITY IN THE ARMY AT ST. LUCIA.

Strength of the Garrison.	The different Months.	Men in Garrison for Duty.	In Gen^{l}. Hospitals.				Prevailing Diseases.	Number of Sick.	Monthly Deaths.	In Regtl. Hospitals.				Prevailing Diseases.	Number of Sick.	Monthly Deaths.	Total Sick.	Total Deaths.
			Fevers.	Agues.	Fluxes.	Dropsy.				Fevers.	Agues.	Fluxes.	Dropsy.					
2325	May.	1784	50	14	36	5	Fever.	105	43	61	56	313	6	Flux.	436	16	541	59
1737	June.	1093	57	9	43	4	Do.	113	68	65	56	413	0	Do.	534	68	644	136
*1912	July.	1012	42	8	51	1	Flux.	102	68	105	94	589	10	Do.	798	125	900	193
1989	August.	1084	34	19	83	8	Do.	144	110	127	202	427	5	Do.	761	142	905	252
1582	September.	899	33	29	86	10	Do.	158	101	100	161	261	3	Do.	525	152	683	253
1533	October.	837	27	29	77	5	Do.	138	56	179	183	196	0	Do.	558	156	696	212
1401	November.	801	25	38	64	4	Do.	131	74	87	166	214	2	Do.	469	65	600	139
1286	December.	883	21	28	49	2	Do.	100	54	60	77	164	2	Do.	363	51	403	105
1268	January.	942	11	38	60	1	Do.	110	69	51	70	104	1	Do.	230	32	325	101
†1540	February.	1230	14	20	67	3	Do.	104	41	47	69	104	0	Do.	220	40	310	81
1554	March.	1233	4	12	49	3	Do.	68	32	77	80	93	3	Do.	253	32	321	64
1442	April.	1172	12	21	33	0	Do.	66	15	72	68	71	1	Fever.	212	29	278	44
			Died in the Gen^{l}. Hospital - - -						731	Died in Regtl. Hospital - - -						908	Total.	1639

* 46th and 5th Regiments joined.

† 37th Regiment joined.

It is evident that the severity of the symptoms, in the Zealand fever, added greatly to the difficulty of the cure; and there could be no opportunity of employing Peruvian Bark or other specific remedies, till its violence had abated, and the redundant bile had been carried off. The treatment of this acute state, consisted chiefly in giving such remedies as purged freely; and in selecting them, the preference was due to those which acted most readily on the liver and the bile, such as calomel; those which were least heating, as the neutral salts; and such as were best borne by the stomach, which, in a great many cases, was extremely irritable. In the course of the general inspection, in which my duty consisted at this time, I had not myself an opportunity of directing and watching the practical details of individual cases; but I had considerable experience in this way, in my attendance on officers in England, who either brought the complaint with them, or were seized on the passage, or after their return to this country.

One of the medical controversies respecting the cause of intermittents, is founded on the difference of opinion on the question, how far the excess of bile may be considered as the cause of them. It certainly cannot, in correct language, be called the cause; otherwise every case of redundant bile, such as the *cholera morbus*, would be attended or followed by an ague. Certain it is, however, that epidemic intermittents not only occur exclusively

in those seasons in which an excessive secretion of bile is most apt to arise, but every attack, whether original or relapsed, which I have seen, bore evident marks of an excessive flow of this humor. The true statement of the fact perhaps is, that that state of the body, in which there is the strongest tendency to a copious secretion of bile, either from the natural constitution, or the season of the year, constitutes a predisposition favourable to the action of the poisonous exhalations. There seems in this something analogous to other facts mentioned in a former paper respecting the Plague and the Yellow Fever, namely, that the human body is not liable to be affected by them, unless when predisposed by a certain temperature of the atmosphere. If the attack of this disease depended merely on the quantity of the exhalations, they would be most frequent in June and July, when the heat of the atmosphere is highest. But there is a still more decisive proof of its depending on that season, in which the secretion of bile is most copious, from this fact, that when those who have imbibed the poison, are transported into countries where the air is in a state of greatest purity, it is in the autumnal months, that they are most commonly attacked. There was a very striking proof of this after the campaign of North Holland in 1779. In the following year some of the officers and men who had escaped the disease, were taken ill in the autumnal months; and none that I heard of, at any other season of the year.

The greatest difficulties which occurred in the cure of those severe intermittents in their early stages, proceeded from the great irritability of stomach, which rendered it very difficult to exhibit either purgatives with a view to procure intermissions, or bark in sufficient quantity after intermissions had been procured. The best means, I found, of obviating the first difficulty, was to purge with calomel, which, besides the advantage already mentioned, is, on account of its small bulk, swallowed without repugnance, and, by its weight, is not easily rejected after being swallowed; and in case the stomach rejected neutral salts, to assist its operation by considerable quantities of carbonated magnesia given in effervescence with lemon juice. It is sometimes advisable to give mercury as an alterative. Ramazini * relates, that a person affected with an obstinate ague was cured by mercurial friction administered for the lues venerea. The second difficulty was obviated by substituting opium and arsenic for bark. The stomachs of some patients were reconciled to the bark, by administering it with opium or magnesia in effervescence, or both; to others it was so insuperably offensive, that it could not be borne in any form, quantity, or combination. In these cases, the cure was effected by opium and arsenic, along with such bitters and aromatics, as the stomach would bear. Where the periodical paroxysms had not ceased, the tincture of opium was given from 30 to 50 drops, in the intermission,

* De morbis artificum.

a few hours before the expected hour of seizure, accompanied with as much rhubarb as would counteract its restringent effects. Sometimes the first administration of this stopped the paroxysm; but more commonly only alleviated it, and did not stop it till the second or third time. After the paroxysms were stopped, it was continued in smaller doses at the former periods; and either bark, or, if the stomach would not bear it, arsenic* was given in the intervals, till it might reasonably be supposed that the tendency to relapse had ceased. At this period, carbonate of iron was also given with safety and advantage, and with still more benefit at a more advanced period, in order to obviate debility and emaciation, and to afford a still greater security against relapse, when there were no remains of fever, nor suspicion of local affection. I have cured intermittent fevers in which bark had failed both in the West Indies†, and in St. Thomas's Hospital, with the oxide of zinc; but I have made little use of this remedy since I became acquainted with the superior powers of opium and arsenic.

The duration of this tendency to relapse, was very indefinite. There is a subtle, incomprehensible impression made on the living human body by marshy exhalations, which, though attended with no

* The dose was from six to twelve drops of the liquor arsenicalis of the London Pharmacopœia, three times a day.

† See Observations on the Diseases of Seamen, page 442. Third Edition.

immediate visible effect, so modify the constitution, that many months afterwards, though the person has been living all the while in a pure air, an intermittent fever arises sometimes, without any visible exciting cause; but most frequently in consequence of cold, fatigue, watching, privation of some kind, or, as has been before mentioned, on the return of the autumn. This, as has been already remarked, was strikingly exemplified in the troops who had served in the campaign in North Holland in September and October, 1799. Among these, was an officer who came to town to put himself under my care, in the month of August in the following year. He belonged to an encampment at Swinly near Windsor, a district not liable to such complaints; and he informed me, that not only himself, but others who had not been affected in Holland, had been seized with intermittents, and that this disorder was confined to those who had been in the abovementioned campaign. I was informed in February, 1811, by a field officer, who came home from Portugal on account of bad health, that those men of his own regiment as well as of others, who had served before in Walcheren, were, upon the first exposure and fatigue, rendered unfit for duty, chiefly by remittent fevers, so as to leave not more than a third part of them fit for service. Here there was a proportion of sick, far above that of the army in general. This tendency is still stronger, if the person had actually suffered from immediate exposure to these exhalations; a consi-

deration which obviously suggests the necessity of continuing the remedies for a considerable time after all the symptoms of the complaint have subsided, and also of avoiding the exciting causes above enumerated.

I had, in the course of this service, an opportunity of observing the extent to which the noxious exhalations extended, which was found to be less than is, I believe, generally known. Not only the crews of the ships in the road of Flushing were entirely free from this endemic; but also the guard-ships which were stationed in the narrow channel between this island and Beveland. The width of this channel is about 6000 feet; yet, though some of the ships lay much nearer to one shore, than to the other, there was no instance of any of the men or officers being taken ill with the same disorder, as that with which the troops on shore were affected.

I had an opportunity of farther proving and illustrating this observation, in the service I was sent upon to Northfleet in the autumn of the following year. The spot upon which it is intended to erect the proposed dock-yard and arsenal, is a marsh of about 700 acres. On the banks of the river, both above and below it, there is soil of a similar description, but not immediately adjoining to it on either side; for above is the village of Green Hithe, which stands on a chalky bottom,

rising to a few inches below the surface, and is a projecting point of the general chalky hills which compose the adjacent country. Below it, on the bank of the river, there is a similar intervention of the chalk, where the village of Northfleet stands. Both these are nearly on a level with the marsh; yet the intermittent fevers are almost unknown at either of them, whereas they are extremely prevalent on the adjacent hills. I found this fact analogous to some others to which my enquiries at this time led me. Dr. Maton informed me, that in the neighbourhood of Weymouth, though there is stagnating water near the sea, producing intermittents, these disorders are not known in the dry districts on each side, on a level with the water, but prevail on the adjacent hills. A Cornish gentleman stated to me, that at St. Blazey, between St. Austle and Lestwithiel, agues prevail much on a hill adjoining to a marsh contiguous to the sea beach. And Major Rennel, the celebrated geographer, says, that in a district which he surveyed on the river Burrampooter, the waters of which overflow, and, upon retiring, leave an oozy flat, the agues prevail to the very summit of the adjoining hills. Lancisi mentions a hill, on which the same sickness prevails, as in the marshy lands at the foot of it*. An instance of the same fact in St. Lucia, has been already mentioned.

* Vid. Lancisi de Noxiis Paludum Effluviis, page 120, Roma 1717.

It is known to every one, ever so little acquainted with the operations of nature, and indeed the common phenomena of clouds and rain render it obvious to the most ordinary observer, that water, recently exhaled from the surface of the earth, has a tendency to ascend, and being lifted over parts on the same level, impinges on the neighbouring heights. There is reason to believe that impure and unwholesome particles in general are attracted by watery vapors, for it is remarkable, that, in case of fogs, offensive smells are perceived, which in a dry state of the air were fixed and quiescent. Though pure humidity, therefore, is innocuous, it may prove pernicious as a vehicle of unwholesome volatile matter. In like manner, the poisonous principle of marshes, whatever it is, being engendered by moist soils, will naturally adhere to the watery vapors, and ascend with them.

There are facts to prove, that certain artificial changes tend greatly to improve the air of particular spots. It is well ascertained, by the records of physic, by the bills of mortality, and by civil* history, that intermittent fevers were very prevalent in London, before the formation of common sewers and the adoption of other means, such as paving, conducive to cleanliness and dryness, to which, more perhaps than to the improved habits of life, in point of diet, may be ascribed the unexampled

* King James the First, and Oliver Cromwell, both died of agues contracted in London.

state of health in this great metropolis. There is a still stronger proof and illustration of this in Portsmouth, which is built upon a flat, composing part of the marshy island of Portsea. I am assured by a medical gentleman who practised there, but is now retired from practice, that when he first knew that place, intermittent fevers were very prevalent; but the town having been drained and paved in the year 1769, that disorder has since been unknown there*. Hilsea and other parts of the Island of Portsea have retained the same aguish character; but this disease has greatly decreased there also, since a drainage which was made in the year 1793. Numberless other examples might be adduced in proof of this, derived from the general improved state of health in various parts of the kingdom, in consequence of the inclosure of commons for the purpose of agricultural improvements, of which draining is one of the principal. This has been felt on the spot now in question, for I am assured by the Rev. Mr. Crackhilt, who has resided in the parish of Northfleet for 42 years, that there has been

* It appears from the late Parliamentary Report, that Portsmouth has had an accession to its population, during the last ten years, of 8401 inhabitants; that the healthfulness of it has encreased, the proportion of deaths in 1800 having been one in 28; in 1810, one in 35; both computations being taken on an average of three years. Plymouth in the same time has acquired an additional population of 12,866, and the mortality has varied but little, having been one in 27 in 1800, and not quite one in 28 in 1810. The population of Portsmouth, by the last Report, was 40,567; that of Plymouth, 56,060.

in that time a progressive amelioration in point of health.

It is mentioned by Bishop Burnet, in his History of the Reformation, that in the last year of Queen Mary's reign, " Intermitting fevers were so universal and contagious, that they raged like a plague;" and we learn from Sydenham and Morton, that intermittent fever was one of the most prevalent and fatal disorders in London from 1661 till 1665, and that for some years afterwards this complaint was very rare. This was probably owing to the greater dryness of the streets by draining, when the city was rebuilt after the great fire of 1666. We are told however by Sydenham, that intermittent fevers revived before the end of that century, and were epidemic from 1677 to 1685. They prevailed a good deal during the first part of the 18th century; and we learn from a work of Dr. Fothergill's, that they occurred as an annual epidemic in the spring and autumn, as late as the years 1751, 1753 and 1754. For more than 30 years past, according to my own observation and the best information I can gather from others, this disease has not been known as an epidemic in this metropolis. I was physician to St. Thomas's Hospital, from the year 1783 till 1795, during which period, the whole number of intermittents that fell under my care, was 192. As there were three physicians, this may be reckoned the third part of the whole admissions for twelve years in an hospital containing 400 patients. I

have not noted in my journal, from what quarter they came; but my memory perfectly warrants me in affirming, that the great majority of them were labourers from marshy districts, particularly Kent and Essex, and there is this internal proof of the greater part being strangers, that, of the number above specified, only 33 were females. Had they belonged to the resident population, the number of each sex would have been nearly equal. On referring to the notes, which I keep of my private practice, I find that in the course of 25 years, I have met with 63 intermittents. Of these, 12 belonged to the armies, that had served in Holland or Zealand, and of the number affected in England, more than one half came from the aguish counties. Several of the cases of those who belonged to the resident population, were so slight and irregular, as to render it doubtful whether they were strictly referable to this *genus* of disease.

One of the objects prescribed to me on my visit to Northfleet, was to ascertain how far the health of that spot might be affected by the exhalations from the Essex side of the river. What has been already said on the subject relating to Zealand, affords an answer to this question; the width of the channel between Walcheren and Beveland being about six thousand feet, and the breadth of the river at Northfleet, according to a plan in the possession of Mr. Rennie the engineer, being three thousand feet. The distance of Essex from the

bank of the river at Northfleet, is therefore about the same as between the ships riding in the middle of the channel, between the shores of Walcheren and Beveland. I found, however, from the most accurate enquiry, that the endemic fever had not spread, either to the ships of war in the roads of Flushing, or to those stationed between the islands, though some of them were nearer the shore than the middle of the channel.

I was further informed by Mr. Rennie, that in boring the ground at Northfleet, he found that there were beds of chalk and gravel underneath the clay, so that these materials, when thrown up in making the excavations, would render the surface dry and wholesome; and that he had calculated their quantity would be such, as to raise the artificial surface eighteen feet higher than the present natural surface.

Taking into consideration, therefore, the great changes which would take place in the marshy spot on which it was proposed to erect the docks and arsenals, in consequence of the excavations, the drainings, the pavings, buildings, and various other operations for forges and other machinery, I gave it as my opinion, that no solid objection, on the score of health, would arise to the plan proposed.

APPENDIX

CONTAINING

Remarks on the comparative Health and Population of England and Wales, at different periods.

SINCE the preceding article was submitted to the Society, the Speaker of the House of Commons has done the author the honor of sending him a copy of the returns of the population and parish registers, made in pursuance of an Act of Parliament past last year. From these, some interesting and authentic information may be gathered respecting the public health, particularly with regard to the disease, which is the subject of this article.

It appears from these returns, that the mortality in England and Wales in the year 1810, was about 1 in 49, the whole population being 10,150,615, and the number of burials 208,184, that the births were in the proportion of 1 in 34, and that every 10 marriages produced on an average 42 children.

The counties in which the mortality was above the average, were Middlesex, where it was 1 in 36; Kent, where it was 1 in 41; Warwickshire, where it was 1 in 42; Cambridgeshire, where it was 1 in 44; Essex, where it was also 1 in 44; Surrey, where it was one in 45; the East Riding of Yorkshire, where it was 1 in 47; Lancashire, where it

was 1 in 48. Of these eight counties, four are subject to agues, namely, Kent, Essex, Cambridgeshire, and the East Riding of Yorkshire, comprising all the counties of that description, except Lincolnshire, in which the mortality is below the average, being 1 in 51. The smaller degree of mortality in this last, is no doubt owing to the great proportion, which the dry and upland part of it bears to the fenny districts. That there is a great difference in the mortality in these, is proved by their respective returns. The mortality in the town of Boston, for instance, which is situated in the fens, is 1 in 27; whereas that of Stamford, which is in the dry and upland division, is 1 in 50.

And here it may be proper to advert to an observation grounded on a very satisfactory induction of facts, in a tract lately published, of which Dr. Wells is the author*, that Phthisis Pulmonalis is but little known in those districts which are infested with the exhalations producing intermittent fevers. But as Phthisis Pulmonalis forms one of the principal heads in the general mortality of England, so much the more is to be imputed to intermittents, in those districts in which they are endemic.

It may be asked, whence arises the greater mortality of the other four counties, of which the rate is

* This tract is an article in Third Vol. of a work entitled, Transactions of a Society for the improvement of Medical and Chirurgical Knowledge. London 1812.

above the average. With regard to Middlesex, it is imputable no doubt to the various circumstances adverse to health, peculiar to the metropolis, such as the more intemperate habits of life, and perhaps still more the unfavorable influence of the air of this great city, particularly on young children. It is worthy of remark, however, that London has of late years been improving in salubrity; for it appears by the bills of mortality, that the burials invariably and considerably exceeded the christenings, till a few years before the close of the last century: whereas since that time the christenings have generally exceeded the burials. This may in part be ascribed to vaccination; but it cannot be entirely owing to this cause, for the decrease of burials took place some years before that admirable discovery. The first year on the records of the bills of mortality, in which the births exceeded the burials in this metropolis was 1790. The decrease in the deaths of children under two years of age, is the most striking point of difference in these tables. In the beginning and middle of last century, their annual amount fluctuated from nine to ten thousand. Towards the end of last century, and during that part of the present which has elapsed, they have fluctuated from five to six thousand: and considering that the computed number of inhabitants in 1700, was only 674,350, and in 1810, by the late enumeration it is 1,050,000; it is evident that the relative mortality in this class, is little more than one third of what it was a hundred years

ago. In farther proof of the improving health of London, it is stated in this Parliamentary Report, that the annual mortality in 1700, was one in 25; in 1750, one in 21; in 1801, and the four preceding years, one in 35; and in 1810, one in 38. The increased mortality in the middle of last century, has been imputed to the great abuse of spirituous liquors, which was checked about that time by the imposition of high duties. The other causes of superior health, seem to consist in a general improvement in the habits of life, particularly with regard to ventilation and cleanliness, greater abundance and better quality of food, the improved state of medicine, and the better management of children.

The high proportion of mortality in Surrey, is no doubt owing to its containing a portion of the metropolis consisting of a population of 170,000, which is more than one-half of the whole county.

The high rate of mortality in Warwickshire, seems at first sight the most difficult to be accounted for, the air of this part of the kingdom being very salubrious. It is no doubt owing to the town of Birmingham being situated here, for it comprises two-fifths of the population; and the mortality on the average of the last ten years, is one in 34. The mortality in this town is greater than in Manchester, Leeds, or Norwich, the ope-

rations in metals, being probably more pernicious to health, than the operations of weaving*.

With regard to Lancashire, where the mortality is somewhat above the average, the number of large towns and extensive manufactures, affording a greater proportion of artisans, to rural inhabitants, than in any other county, except those in which the metropolis is situated, is certainly the cause of this, for the air is very salubrious, and the great quantity and cheapness of fuel, is extremely friendly to life, health, and comfort. The report of Manchester, which is the second town in England in point of population, forms an exception to this remark, for the mortality there on the average of the last ten years, is one in 58, and in the year 1810, one in 74. But that of Liverpool, is one in 34 on the average of ten years, and one in 30, in the year 1810. In the former we have another pleasing picture of the progressive improvement of health, for it is stated by the late Dr. Percival, that in the year 1757, the annual mortality of Manchester, was one in 25.7, and in 1770, one in 28; although at the former period the population was not quite one-fourth, and at the latter period, not one-half of its present amount†. This improvement of health is greatly imputable to certain regulations of police, particularly with respect to

* See Ramazini de Morbis artificum.

† See the Works of Dr. Thomas Percival, Vol. II. Warrington, 1789.

ventilation, recommended and introduced by the above mentioned benevolent, enlightened, and active physician.

The like progressive amelioration of health, is deducible from these public documents with respect to the whole kingdom, as has been remarked locally; for it appears from the returns of 1800, that the mortality of all England and Wales was then one in 45; but in 1810, it proves to be one in 49.

This statement of facts coupled with the general result of the population returns, from which it appears, that this island has acquired an addition of 1,536,000* inhabitants in the last ten years, together with the annual increase of wholesome subsistence from the rapid extension of agriculture, may be fairly deemed a proof of the increasing happiness, power and prosperity of this country, and cannot fail to afford the most solid satisfaction and delight, to every benevolent and patriotic mind.

* According to the last returns the population of Great Britain was 12,596,803 persons, of whom 6,334,087 were males, and 6,262,716 were females. It farther appears, that when classed according to their occupations, 895,998 families were engaged in agriculture, 1,129,049 were engaged in trade, manufactures, or handicrafts, and that the number of families comprised in neither of these classes was 519,168.

HISTORY

OF A

REMARKABLE CASE

OF

OVARIAN DROPSY.

BY THOMAS CHEVALIER, Esq. F. L. S.

SURGEON EXTRAORDINARY TO THE PRINCE REGENT; AND SURGEON TO THE WESTMINSTER GENERAL DISPENSARY.

Read December 24, 1811.

ON May 31, 1811, I was requested to visit Maria Grant, a young woman, 23 years of age, of a spare habit, who was confined to the house by ovarian dropsy, which had enlarged the abdomen to a size very far exceeding any thing of the kind I had ever seen. The disease began about six years before, by a tumor in the left side, unaccompanied by any derangement of her general health, which was even now very good, excepting only that she had not menstruated for two years past, and that the secretion of urine was scanty. Dr. Simmons had from time to time prescribed for her; but as medicine was not productive of any advantage, he wished me to see her.

I found the abdomen to measure 63 inches and a half in circumference, and 38 inches from the

point of the ensiform cartilage to the top of the pubis. The legs were œdematous; the left considerably more so than the right; and great part of the skin of the left leg was in a state of complete ichthyosis. The lower part of the belly was also œdematous, and the navel, when she sat, was on a line with the knee.

The enormous quantity of fluid accumulated, made it appear to me improper to think of evacuating the whole at once, and I judged it best that the first evacuation should be made in a very gradual manner, lest a hasty diminution of the tension should produce syncope, and occasion too great a disturbance of the system. I called therefore on the following day, June the first, accompanied by Dr. Merriman and Mr. Copeland, and made an oblique puncture with a lancet at the most prominent part of the abdomen, in the linea alba, six inches above the navel; and introducing a director, evacuated seven gallons and a half of very dark, coffee-coloured fluid. When this quantity had been discharged, she began to feel somewhat faint, and I immediately closed the puncture with adhesive plaster and graduated compresses. The abdomen was still tense, and measured $55\frac{1}{2}$ inches in circumference, and $31\frac{1}{2}$ from the ensiform cartilage to the pubis.

She passed a good night, and on the following day appeared much relieved. The bowels acted

properly without any assistance from medicine; and she passed near twice as much urine in 24 hours, as she had been accustomed to do for several years before, and this increase in the secretion of urine continued.

June the eighth, the enlargement appeared to be considerably more on the left side. The abdomen measured 54 inches round, of which 30 inches were from the spine to the middle of the linea alba, on the left side, and 24 inches on the right. From the point of the ensiform cartilage to the pubis were 30 inches, so that a diminution of an inch and an half in each direction had taken place in the interval between the operation, and this period. I now made a second puncture in the most prominent part of the left side, and discharged four gallons and an half of straw-coloured, ropy fluid. She then became languid, and the opening was closed as before. The abdomen now measured 48½ inches round, and 27 from the ensiform cartilage to the pubis: a bandage was applied.

No disorder was occasioned by this puncture. The parietes of the belly gradually contracted so as to render it less flaccid; the quantity of urine she passed was as much as she had ever used to void before the disease commenced; and the size of the belly continued to lessen, so that on the 27th of June the circumference was only 45 inches, and the distance from the ensiform cartilage to the pu-

bis only 24½. On this day I made a puncture with a trocar on the left side; and a gallon and a half of very ropy brownish coloured fluid was discharged. The canula then became obstructed by a very firm cyst pressing against it. It was therefore withdrawn. The circumference was diminished 2¾ inches by this operation, the other measure remaining the same.

July the sixth. The circumference was 42 inches, the distance from the ensiform cartilage to the pubis still 24½. I introduced a trocar which entered the abdomen, and evacuated a small quantity of fluid, but was stopped by a cyst of uncommon firmness which I could not penetrate till I withdrew the stilette, and introduced through the canula the trocar used for puncturing the bladder through the rectum. This entered the cyst, and drew off 3 gallons and an half of ropy brown fluid, and appeared completely to empty the abdomen; through the flaccid parietes of which, the encysted ovarium could be distinctly felt. No syncope followed this, or either of the former operations.

On the following day, July the 7th, her pulse was rather quickened, and her skin somewhat hot; but she was free from pain or tenderness of the abdomen. July the 10th, the circumference was 34 inches, and the distance from the ensiform cartilage to the pubis 17½. Her skin still continued hot, and her pulse frequent. She complained fre-

quently of sickness, her strength failed; she had no relish for food, and in a few days every thing she took except gruel, was rejected, or followed by a stool. Medicines gave only a temporary and imperfect relief, for she languished in this state, with few material variations, till the sixth of September, which was the day of her death. The abdomen had for a time become enlarged during this interval, but had greatly subsided again. Its circumference then being only 34 inches, and the space from the ensiform cartilage to the pubis 17 inches. She had however become extremely emaciated. The circumference of the thorax immediately under the axillæ was only 27 inches.

On examining the body after death, the whole cavity of the abdomen appeared to be occupied by two large cysts, formed in the left ovarium, closely connected together, their coats being of a very firm texture, particularly the lower one, which was much the largest and thickest. They both adhered anteriorly and laterally to the parietes of the abdomen, so as to require considerable force, and some dissection, to detach them. The upper cyst contained about 2 quarts of a brown glairy fluid, like that which had been evacuated in the first operation, with some loose flakes of coagulable lymph. The inferior cyst contained 3 gallons of purulent fluid. Its inner surface was covered with coagulable lymph, having in it many large dark red spots. There were two very small cysts also formed in this

ovarium, one containing about 4 ounces of a soft gelatinous substance, and the other about half an ounce. The right ovarium and the uterus were in a perfectly healthy state.

The right kidney had entirely lost its natural appearance, and was converted into a sac, having two distinct cavities, filled with a straw-coloured fluid, and having no opening into the ureter. The left kidney and the rest of the viscera were sound in their texture. But the liver was unusually small. The whole of the abdominal viscera were forced upward; encroaching on the cavity of the thorax so much, that the summit of the diaphragm was on a line with the upper edge of the third rib.

This case appears to me to afford several interesting particulars. A very obvious one, is the enormous quantity of fluid accumulated. For as the size of the abdomen diminished between each operation, the secretion of urine being in the mean time increased, I am certainly warranted in assuming, that at least a quantity equal to the whole which was drawn off in the four operations, had been collected in the belly at first. This, it will be found, amounts to seventeen gallons; a quantity, I believe, far exceeding that of any other case on record. And if any allowance is to be made for the diminution of size between the operations, it would appear that the original quantity must have been even considerably more than I have now stated.

It is also singular that so very large a collection of fluid should be formed without any derangement of the general health, for the bowels had all along continued their action as regularly as usual. The breathing and pulse too were unaffected, notwithstanding so great an encroachment on the capacity of the thorax. In short, almost the whole inconvenience she appeared to sustain from the disease was the excessive weight she had to carry.

Another circumstance worthy of remark, is the progress of inflammation and suppuration in the larger and inferior cyst, to which her death is to be ascribed. That cyst, near a quarter of an inch in thickness, and as compact in its texture as parchment, was entirely insensible; but it was most curious to observe that the process of inflammation should be excited in it, and go on to so large a collection of pus, attended with a rapid decay of the general health, and showing by that, the sympathy of the constitution with this adventitious substance, without pain or tenderness being excited in it, or the neighbouring parts; for no mark of inflammation whatever was discoverable in the peritoneum, or in any part exterior to this cyst; and as I have already observed, during the whole interval between the fourth operation and her dissolution, she bore the abdomen to be pressed by any degree of force, without complaining of the smallest sensation of tenderness or pain.

A CASE

OF

DIFFICULT PARTURITION,

OCCASIONED BY

A DROPSICAL OVARIUM,

FORMING

A TUMOR IN THE LOWER PART OF THE PELVIS.

By SAMUEL MERRIMAN, M. D.

PHYSICIAN ACCOUCHEUR TO THE MIDDLESEX HOSPITAL, AND THE WESTMINSTER GENERAL DISPENSARY.

Read January 7, 1812.

IN the Volume of the Medico-Chirurgical Transactions just published, I have read with much interest, a paper by Mr. Park, of Liverpool, "on Tumors within the Pelvis occasioning difficult Parturition." The intelligent author does not offer any explanation of the origin or nature of these tumors, but contents himself with giving a very clear and precise relation of the circumstances attending each case. As the subject is deserving of farther investigation, I take the liberty of laying before the Society, the detail of a case I at-

tended several years ago, which will, I think, throw some light on Mr. Park's paper.

December 20th, 1804, I was requested by my friend, Mr. Robertson, surgeon, then residing in Half-Moon Street, Piccadilly, but now at Great Hindred in Berkshire, to see a poor woman in labour, to whose assistance he had been called the preceding evening. I learnt that she was about forty years of age, that she had been delivered of her first child in the country, eight years before, since which she had never been pregnant till now. Her health was not very good, and she was very apprehensive respecting the result of her labour, for her former one had proved very long and difficult, though the child was extremely small. The child however was born alive, but died in a few hours.

Mr. Robertson informed me, that the *Liquor Amnii* had been evacuated before he was sent for, and that the labour had made very little progress during the last eight or ten hours, though the pains had been very frequent and severe: he likewise told me that there was a tumor of an uncommon nature in the vagina, on which account, principally, he wished me to see the patient.

Having introduced my finger within the vagina, I felt a soft elastic tumor, which seemed capable of containing four or five ounces of fluid: it was compressible, and did not give the sensation of

fluctuation under the finger, but rather felt as if it were a large pouch, formed by the coats of the rectum preternaturally distended. On passing my finger beyond this tumor, I felt the child's head within the os uteri, which was thick, rigid, and very little dilated. I was not unacquainted with the fact, of the ovaria in a diseased state having been occasionally found lying between the rectum and the vagina, and it occurred to me that this might be a case of that kind; but I must confess, that I too readily abandoned this idea, being prepossessed with the opinion, that the disease was situated in the coats of the rectum.

From the state of the os uteri, it was apparent that nothing could be safely undertaken towards effecting the delivery of the patient at present; but as there was great reason to believe, that the bowels were much loaded, we determined to empty them by castor oil and clysters, and afterwards to give a large dose of laudanum, in order to quiet the pains, which teazed the patient excessively, without producing any advantage. By these means very copious evacuations from the bowels were produced, and some sleep was procured; but the os uteri remained undilated, and the bulk of the child's head was still above the superior aperture of the pelvis.

It will be unnecessary to state minutely all the circumstances of this labour, while we were wait-

ing for the natural dilatation of the os uteri, and more effectual uterine action, which, we felt assured, would overcome all the difficulty of the case without extraordinary assistance, for the tumor did not seem firm enough to afford much resistance to the birth. It is sufficient to say, that after an interval of thirty-six hours, passed in much severe but ineffectual pain, during which the poor woman's strength was much impaired, we judged it absolutely necessary to have recourse to the perforator. We found more difficulty than we had expected in using this instrument, and afterwards in loosening the parietal bones, in consequence of the size of the vaginal tumor; and after the head was diminished, a considerable space of time elapsed before the child could be brought into the world.

Soon after this child was born, it was ascertained that there was another in the uterus, which being smaller was expelled by the natural pains: it appeared to have been dead some hours. The placentæ came away soon afterwards, and the woman was put to bed in a very exhausted state, from the length and severity of her labour.

On the 22d, the day after delivery, she was so much better, that we began to hope she might do well; but unfortunately, her circumstances admitted of but few of those comforts which her situation rendered necessary, and her attendants,

notwithstanding all our injunctions to the contrary, were more disposed to indulge her with beer-caudle and strong liquors, as a means of supporting her strength, than to pursue the plan of treatment which we had directed. This improper indulgence, added to the severity of her labour, brought on peritoneal inflammation, and on the 25th she died.

The body was opened the next day, and exhibited the usual appearances of peritoneal inflammation. The left ovarium was in its proper situation, and of its usual size; but the right ovarium was found lying between the rectum and the vagina, and had formed the tumor which we felt during the labour. It was about the size of a small trap-ball, and in a very high state of inflammation. It was divided into several cavities by membranous *septa*, which had a scirrhous feel, and was somewhat more than half filled, with a fluid resembling, in colour and consistence, honey and water mixed together: it contained likewise a clot of blood, which must have been forced out of the vessels by the pressure of the child in passing, or by the violence of the pains.

The pelvis was narrow, the diameter from the symphysis pubis to the sacrum being but little more than three inches; this alone therefore would have delayed the passage of the head through the superior aperture. It is not improbable that the

presence of twins in the uterus might likewise prevent the full effect of the pains; but I imagine, that the position of the diseased ovarium, more than any thing else, prevented the expulsive efforts of the womb; not because it proved an obstacle to the birth of the child, (for the child did not come in contact with the tumor till after the perforator had been used,) but by paralysing, if I may so express it, all regular uterine action.

The fatal termination of this case made me resolve, should another of the same kind occur in my practice, that I would endeavour to evacuate the contents of the tumor, by puncturing it through the vagina, and I am well pleased to find from Mr. Park's cases, that this may be done with so great a probability of success. It must however be recollected, that ovarian tumors not containing a fluid, may sometimes be found in the pelvis. I have in my possession, a preparation consisting of the ovarium enlarged, and filled with fatty matter intermixed with hair, which has formed to itself a bed, between the rectum and the vagina. Had the woman from whom these parts were taken, become impregnated, the ovarium must of necessity have been confined in this situation, and a tumor occupying a considerable space, must in consequence have been formed in the pelvis, which would have proved a great obstacle to parturition.

A case of this kind has lately occurred in one of the Lying-in-Hospitals of this city, as I was informed by a surgeon, who was present at the dissection. The birth of the child having been long delayed by a tumor in the vagina, it was determined to lessen the child's head, and the woman was delivered, but did not long survive. After death it was ascertained, that the tumor was occasioned by the ovarium filled with fatty matter intermixed with hair and teeth, lying between the vagina and the rectum.

In Baudelocque's "Art des Accouchemens," § 1963, a similar, and, as he supposes, a solitary, case of diseased ovarium in the pelvis, impeding the birth of the child, is recorded. It had been proposed by Lauverjat, and another accoucheur of Paris, to perform the Cæsarean operation, as the only means of delivering the woman. Baudelocque objected, and recommended delivery by the feet. After several ineffectual attempts on the part of the other accoucheurs to turn the child, by which the tumor was dislodged, and forced above the brim of the pelvis, Baudelocque succeeded in bringing down the feet, but was obliged to have recourse to the forceps, in order to accomplish the delivery of the head. The child was dead born, and the woman died in two days. On opening the body, the tumor was found to be occasioned by the ovarium enlarged, and filled with

long hair and well formed teeth, among which could be easily detected incisors, canine teeth and molares.

It becomes a question whether under the circumstances, an incision should be made into the tumor with the view of extracting the hair, &c. As these heterogeneous substances are usually contained within a capsule, might it not be possible to remove the whole mass by an incision? or might not the ovarium itself be extirpated, through the posterior part of the vagina? Either of these operations might be practicable without much additional hazard to the mother, and with a great probability of preserving the child. That extirpation of the ovaria is not necessarily a fatal operation, has been proved by many experiments upon brute animals; and in Mr. Pott's Works, Vol. III. p. 352, a case is recorded of the extirpation of both the ovaria in a woman, where they had passed through the tendinous openings of the oblique muscles on each side. The patient recovered, but lost the fullness and fleshiness of the breasts, and never menstruated afterwards. Operations indeed on any parts connected with the uterus, cannot be otherwise than dangerous, during pregnancy, and especially during labour; yet if the ovarian tumor were so extensive, as either to impede the descent of the child's head by its bulk, or to prevent the action of the womb by its weight, or by its con-

fined and imbedded position in the pelvis, the safety of both mother and child might be promoted by the operation. It must be considered, that the forceps or the lever can very rarely be employed with effect, when the vagina is thus protruded by a tumor behind it; the only instruments that we can look forward to, are the perforator and hook; the use of which presupposes the death of the child, if not its actual sacrifice, at the moment of employing them. And with regard to the mother, these instruments cannot be had recourse to in such cases, without exposing her life to great risk, as has been proved in several instances. Besides the two already mentioned, in both which the mothers died, a similar case is related by Dr. Denman*, where the same fatal event followed the use of the perforator.

Nor does it appear, that turning the child, and delivering by the feet, is safer practice, as is proved by Baudelocque's case, and likewise by one somewhat similar, attended by Doeveren†, in both of which the mothers and children perished. Upon the whole, therefore, I am disposed to believe, that where the tumor in the vagina occupies a large space, it would be a more warrantable practice, to remove it by excision, if it consisted of a

* Introduction to the Practice of Midwifery, 4to edit. p. 324.

† Doeveren Specimen Observationum Acad. cap. 12. Groningæ, 1765.

solid substance*, and certainly to puncture it, if it contained a fluid, rather than to expose the child to certain death, and the mother to great hazard, by employing the perforator.

Dec. 20th, 1811.

* Dr. Drew removed by excision during labour, a tumor of a "fat, grisly substance," growing from the sacro-sciatic ligaments, fourteen inches in circumference, and weighing two pounds eight ounces; which entirely prevented the birth of the child. The child was born alive, and the mother perfectly recovered.

Edinburgh Medical and Surgical Journal, Vol. I. p. 20.

CASE

OF

DISEASED

APPENDIX VERMIFORMIS,

By JOHN PARKINSON, Surgeon, Esq.

COMMUNICATED

By JAMES PARKINSON, Esq.

Read January 21, 1812.

A preparation of diseased appendix vermiformis in my possession, was removed from a boy about 5 years of age who died under the following circumstances.

He had been observed for some time, to decline in health, but made no particular complaint, until two days before his death, when he was suddenly seized with vomiting, and great prostration of strength. The abdomen became very tumid and painful upon being pressed: his countenance pale and sunken, and his pulse hardly perceptible. Death, preceded by extreme restlessness and delirium, took place within 24 hours.

Upon examination, the whole surface of the peritoneum was found inflamed, and covered with a

thin coat of coagulable lymph; and slight adhesion had taken place between the peritoneum covering the viscera, and the parietes of the abdomen. The viscera, independent of the inflammation of their peritoneal covering, appeared in a perfectly healthy state, excepting the appendix vermiformis of the cœcum. No diseased appearance was seen in this part near to the cœcum; but about an inch of its extremity was considerably enlarged and thickened, its internal surface ulcerated, and an opening from ulceration, which would have admitted a crow quill, was found at the commencement of the diseased part, about the middle of the appendix, through which it appeared, that a thin, dark coloured, and highly fetid fluid, had escaped into the cavity of the abdomen.

Upon opening the appendix, a piece of hardened fœces was found impacted in that part of it which lay between the opening, and that portion of the appendix, which was not evidently marked by disease.

A CASE

OF

DISEASED TESTICLE,

ACCOMPANIED WITH DISEASE

OF THE

LUNGS AND BRAIN,

AND TERMINATING FATALLY,

By HENRY EARLE, Esq.

COMMUNICATED BY

WILLIAM LAWRENCE, Esq.

TO WHICH IS ADDED

A NOTE,

BY MR. LAWRENCE,

CONTAINING SOME PARTICULARS OF THE HISTORIES AND DISSECTIONS OF FOUR CASES.

Read February 3, 1812.

THOMAS DENNIE, aged one year and nine months, was brought to me on the 10th of June 1811, for my opinion on a disease in his left testicle. The following is the history which his mother gave me of its origin and progress. When the child was about a year old, he received a pinch from his sister who was nursing him; he complained a good deal at the time, but not much attention was paid to it. About a fortnight from this time,

his mother first perceived that the left testicle was rather larger than the right. From this period it gradually encreased until it attained the bulk which it exhibited when she came to consult me. She had applied to several surgeons, who recommended leeches, poultices, mercurial ointment, &c. &c. without producing any visible amendment. The case had been twice mistaken for a hydrocele, and the testicle had been punctured with a trochar, but no fluid followed. Very little inflammation succeeded the operations, and the disease did not appear to have been materially aggravated by them. At the time when I saw the child, his testicle was rather larger than a goose-egg, and, when unsupported, reached to the internal condyle of the femur. It was of an oval figure, with a regular smooth surface, and when pressed had an elastic feel, so as to produce the sensation of a fluid contained in a cyst: so deceptive was the feel, that a gentleman, who has had most extensive practice in the treatment of hydrocele, pronounced it without hesitation to be a case of that description. It was not, however, in the slightest degree diaphanous; and at the same time, was much heavier than a similar bulk of water. No testicle or epididymis could be traced at the posterior or inferior part of the tumor. The child had a most unhealthy aspect; his skin was of a greenish yellow colour, and bedewed with a clammy moisture; his muscles flaccid and diminished in size; his head was large and prominent in front, his eyes heavy, and pupil dilated, the iris being of

such an unusually dark colour, as to be with difficulty distinguished from the pupil. His respiration was rather anxious; he was troubled with cough and had a frequent hard pulse. The abdomen was large and tense, and he was habitually costive. I ordered him Hydrargyri submuriatis gr. iij. Jalapæ gr. vj. omni alternâ nocte, and a lotion consisting of Liquoris ammoniæ acetatis, aquæ fontanæ ā ā ʒ iv. to be constantly applied to the part during the day, and a bread and water poultice at night. He continued the use of these medicines for a week, without any alteration in the state of the testicle, his cough was rather better and his abdomen was softer. As the disease had existed for nine months, and had resisted all applications, I did not think that any good would arise from prosecuting these measures, and much evil might be expected from the delay which they would necessarily occasion. I therefore thought it my duty to propose an operation, although from the unhealthy appearance of the child, and the unfortunate result of similar cases, I was not very sanguine of ultimate success; still however I was led to entertain some hope from the circumstance of being able to trace the spermatic cord distinct and free from disease, for nearly an inch above the tumor, and from the inguinal glands being perfectly healthy and natural.

On the 22nd I performed the operation: after laying bare the tumor in its whole extent, I passed a needle and ligature round the artery, close to the

ring of the external oblique, and then divided the cord, in which there was no disposition to retract. The rest of the operation was accomplished in the usual manner. The tumor consisted of a pulpy greyish mass, in which no vestige of the original structure remained; after washing it for some time, the water became turbid, in consequence of part of the mass, consisting of matter resembling brain in a state of putrefaction, being dissolved. The cut surface was uneven and fibrous in appearance, like sloughs or irregularly deposited coagulable lymph. The disease did not extend up the cord; the artery was very large, and the corpus pampyniforme much encreased in size. The nature of the complaint was evidently the same as has been described under the several names of pulpy testicle, medullary sarcoma, and fungus hæmatodes. Nothing worthy of remark occurred during the after treatment, the wound granulated from its whole surface, and healed very slowly, frequently requiring to be stimulated with red precipitate or the nitrate of silver, &c. In six weeks it was perfectly cicatrized. During this time he took the following medicines.

℞ Liquoris Hydrargyri Oxymuriatis ʒ j.
Tincturæ Cinchonæ ʒ j.
Infusi Quassiæ vj. M. ft. mistura.
Capiat cochlearia tria majora bis quotidie.

℞ Hydrargyri Submuriatis gr. ij.
Rhei Palmati gr. x.
Zingiberis gr. x. M. ft. pulvis tertiâ quâvis nocte sumendus.

Under this plan of treatment the health gradually improved. His skin was more natural, and he had even some colour in his face; he was more lively, his bowels acted regularly, and his abdomen was smaller and much softer. Indeed he was so much better in every respect, that I hoped his cure would be permanent. About the 25th of August, I ceased to attend him, and recommended his being sent into the country. I substituted the Haustus Gentianæ cum Senna Ph. S. Barb. for the mixture containing the muriate of mercury; and six grains of Hydrargyrus cum Cretâ for the calomel, and enjoined a strict attention to his diet.

I saw nothing of the child from this time until the 12th of November, when his mother again brought him to me. She had not been able to send him into the country, and had discontinued the use of the medicines soon after I ceased to attend him. She told me he had continued well in every respect until the end of September, when, to use her expression, "he again began to fall off." He was now much worse than when I first attended him, all his bad symptoms having returned with aggravation. He was very prone to sleep, and peevish when roused. He was constantly desirous of resting his head, which was evidently increased in size, although all the sutures were closed, and frequently carried his hands there in his sleep. He made a constant moaning querulous noise, and was very averse to being moved. His pupils were

dilated, but sensible of the impression of a strong light. His respiration I could not observe, as he always cried when I approached him. His pulse was frequent and irregular; his abdomen full, and bowels constipated; and he had disinclination for food. I immediately suspected that the same diseased action was begun in the brain, which had existed in the testicle, and foretold the unhappy termination of the case. I ordered him hydrargyri submuriatis gr. v. jalapæ gr. x. This was repeated twice before it operated, it then produced several copious fetid dark-coloured stools. The following day (the 13th) leeches were applied to his temples, and a seton was cut in the nape of his neck. Two grains of the submuriate of mercury, and six of extract of colocynth, were directed to be taken night and morning. On the 14th he was rather more lively; the leeches and medicine were ordered to be repeated. 15th. He was not so well, was very prone to sleep, and had slight strabismus. I directed his mother to rub a drachm of mercurial ointment into his body night and morning, and to continue his pills as before. The seton was poulticed, and was beginning to discharge. I did not see him again before the 18th, when no material change had taken place. His mother had been ill, and had not rubbed in the ointment above twice. I desired her to bring him to the hospital the following day, and promised to procure his admission, as I was very anxious to watch the progress of the case. She neglected to do this, and as she

had changed her place of abode I was not able to find her out, and did not see him till the 28th. The disease had made rapid progress during the ten days. The head was much enlarged, and the veins of the scalp were very turgid and increased in size. His pupils were dilated, and his countenance expressive of great anxiety. He was still sensible, and the moment I entered the room he cried and clung to his mother, as he was wont to do ever since the operation. His lower extremities were now paralysed, and he passed his excrements involuntarily. His pulse was slow and irregular, with some degree of fulness. His mother said that she had rubbed in the ointment eight times, when his mouth became affected; that his bowels had acted regularly every day; that on the 25th he began to lose the use of his legs, which symptom had gradually increased until all below the pelvis was palsied. He had passed a small calculus of lithic acid the day before. In this hopeless state it was useless to attempt any thing further. I saw him early the following morning. He was then convulsed in the muscles of his face and eyes, which were in a state of constant tremulous motion, and wide open. His upper extremities and all the muscles of his trunk were paralysed. His respiration was difficult, and much impeded by a collection of mucus in his throat. His pulse was too rapid to be numbered. At eight o'clock that night he died, and the following morning I examined his body.

Appearances on Dissection.

On removing the calvarium, nothing particular presented itself in the appearance of the dura mater. On slitting up the longitudinal sinus, the glandulæ Pacchionii were found diseased, having an appearance of grumous blood. After raising the dura mater, the convolutions of the brain were so much flattened as completely to obscure the intervening sulci, and to give the cerebrum an uniform even surface. The vessels of the pia mater were particularly void of blood. In the anterior lobe of the right hemisphere, a large tumor presented itself immediately beneath the arachnoid membrane; its surface was of a dusky red colour, and was rather rough. Two other tumors were distinctly felt in the substance of the brain, one in the posterior lobe of the right, the other in that of the left hemisphere, previous to any section being made. On cutting through the substance on a level with the corpus callosum, four more tumors were exposed to view, and the three former were divided. The largest was of the size of an orange, the smallest as big as a chesnut. They were of a very firm consistence, of a dusky red colour, with streaks of white interspersed through the substance. One of them was of a darker colour on its exterior surface, having more the appearance of a firm coagulum of venous blood. The surrounding cerebral substance was remarkably soft and pulpy, and had a peculiar yellow tinge, which was not

destroyed by letting water run over it. So loosely were these tumors connected with the substance of the brain, that they were with difficulty preserved in their situation. The ventricles were healthy, and plexus choroides pale and void of blood. The cerebellum was quite free from disease, the medulla oblongata remarkably firm, and its processes very strongly marked. On opening the thorax, numerous large tubercles were found in the substance of the lungs. In some places they occupied the whole thickness from one surface to the other. When viewed externally through the serous membrane of the lungs, they had a greyish white appearance, and were irregularly radiated at their circumference. When pressed, they had a firm inelastic feel. On being cut into, they exhibited a close even texture, very dissimilar to that of the testicle; when squeezed, a small quantity of a whitish fluid was pressed out. On tearing one of them, it had a fibrous appearance like a firm brain when torn or dissected with the handle of a scalpel. There was nothing like a capsule to any of them, they were contained in the substance of the lungs, and their edges were not well defined. The bronchial glands had taken on the same diseased action, and were prodigiously enlarged, their internal structure was very analogous to the testicle, though they were of a firmer texture, and were not elastic to the touch previous to being cut into. Not much disease was found in the abdomen. The mesenteric glands were enlarged, but when

cut into did not exhibit any diseased structure. A chain of enlarged glands was traced on the side from whence the testicle had been extirpated, running by the side of the aorta, and extending from the pelvis to the diaphragm. The intestines were much distended with flatus, but not unhealthy in their appearance. The right kidney was enlarged, and its ureter encreased in diameter, by the recent passage of a calculus.

I fear I shall incur the charge of prolixity in the statement of the former part of this case, as I am fully aware that a pulpy testicle is by no means an unusual occurrence. It will, I trust, be excused, as the necessary prelude to the interesting dissection which the termination of the case afforded. Although this is not a very rare disease in the testis, it appears that its diagnostic symptoms are not very well defined, nor its nature generally known, as many cases are on record where surgeons of great and deserved reputation have inadvertently plunged their trochars into the substance of the tumor. Fortunately, no material injury is likely to accrue to the patient from such an operation; but the surgeon will be liable to much censure for his mistake. It is certainly difficult to determine the nature of some cases; but there are generally sufficient characteristic marks to enable a practitioner to form an accurate judgment. The regular oval figure, the peculiar elastic feel, and total want of transparency ought certainly to raise

suspicions in a person's mind, which should render him cautious in giving his opinion. Under these circumstances no man should attempt to puncture the tumor, without being fully prepared to follow up the operation, by removing the testicle in the event of there being no collection of fluid; but any one who has been in the habit of comparing the two diseases, will find much difference even in the sensation imparted to his touch. To say in what this difference consists, I will not attempt, as it is almost impossible to express in words those nice powers of discrimination which can only be gained by experience. An attention to the history of the case, and to the state of health, which always suffers more or less, will be of much assistance in forming an opinion. The term fungus hæmatodes has been applied to this disease, but upon what principle I am at a loss to determine, as there does not appear to be any disposition to the production of fungus, nor any oozing of blood when it is wounded. What the natural termination of the disease would be, supposing it to exist alone in the testicle, has not, as far as I know, been yet determined. It generally spreads to parts essential to life, and destroys before the ulcerative stage has commenced. The term medullary sarcoma is perhaps equally exceptionable, as it does not convey a correct idea of the disease in different parts of the body. In the brain, for instance, where we have the most favourable opportunity of making the comparison, it bears no analogy to the surrounding medullary

matter. It is much easier to object than to amend, and it may be wrong to find fault with a term when we have not any better to offer; but the first step to improvement is the exposition of error. Any term founded on the appearance of this disease, in any one part, must necessarily convey a wrong impression of its nature in other parts, as it is certainly much modified by its situation. I have considered this case as worthy of being recorded, as it shows in a striking manner to what extent a disease may reach in so important an organ as the brain, without materially deranging its functions, provided its progress be gradual. This is the more to be wondered at, when we consider that a total suspension of its powers, and even death, will sometimes ensue from so slight a derangement of its structure, as almost to elude the investigations of the anatomist. I had occasion to make this same remark about two years since, in a case of large abscess in the brain, which was not at all suspected during life. An account of this case was published in No. 132 of the Medical and Physical Journal. I consider the termination of this case as a good illustration of the gradual loss of power in the brain, when labouring under the effects of disease. It is first incapable of transmitting its influence to the extreme parts of the body, as the disease advances its powers gradually diminish, and the extent of its influence diminishes in proportion, those parts nearest the brain being last affected. It is probable that a less morbid state of brain may produce a numbness, or even palsy, of

the lower extremities. This analogy between the centre of the nervous and sanguiferous systems, has not been sufficiently noticed by nosologists, and may perhaps assist in the explanation of many phenomena in diseases hitherto involved in obscurity. It has been a question, whether this disease spreads by absorption, or is constitutional. The present instance strongly favours the latter opinion. Its appearance at such remote parts, where those intervening were so slightly affected, proves that it depends on the same peculiarity of constitution, and prevalence of the same diseased action. Since writing the above, Mr. Hodgson, who was present at the dissection, has favored me with a very accurate representation of the appearance of one of the tumors in the brain. Figure I. represents a section of the tumor, shewing the looseness of its connection with the brain, and the bloody and mottled appearance of its structure. Figure II. represents the appearance of the tumor in the surface of the brain covered by pia mater.

Remarks by Mr. Lawrence.

The occurrence of disease in several organs of the same individual, is a point of so much importance in pathology, and the combination of affections of the internal parts with external disorders is of so much practical consequence, particularly in influencing our judgment concerning the propriety of operations, and in leading to a just pro-

gnosis, that I take the liberty of offering to the Society a few particulars of four cases, which have come under my observation within a very short time. They will form a proper sequel to the preceding very interesting narrative of Mr. Earle.

CASE I.—M. Lambertini, an Italian, about thirty-five years of age, perceived a hard tumor in the right breast about fifteen months before his death. When this had existed about twelve months, it ulcerated, and a fungus shot up. I saw him about three weeks before his death, at which time there was an ulcerated and fungous surface of about two inches in circumference, on the right of the right nipple, which was sound. One considerable protuberance in this surface was ulcerated, nearly smooth and florid: the rest was irregular and tuberculated, and covered mostly with an unhealthy cuticle. The discharge was not considerable, and rather offensive: sometimes it bled slightly. A hard mass occupied nearly the whole anterior part of the right half of the chest: the skin was hard and fixed, and small tubercles were felt under it for some distance round the fungous part. The swelling and the hard state of the skin reached into and filled the axilla; it extended up to the clavicle, as far as the middle of the sternum, and below the edge of the pectoralis major muscle. Hard tumors were felt above the clavicle; and the axillary glands on the opposite side of the body were enlarged. The arm began to be œdematous,

and respiration was affected; he was also remarkably hoarse. The pulse was not affected, the appetite very good, and little if any emaciation had occurred. The œdema and difficulty of breathing gradually increased, debility came on, and death took place without any particular change in the local disease, which had at no time been attended with much pain. The carbonate of iron was used both externally and internally: the discharge became less offensive, and the bowels very costive under its employment.

A very considerable mass of exceedingly hard and tough substance covered the chest, reaching as high as the clavicle, and filling the axilla. It contained no cavity or cell, nor any part at all soft in its texture. The most striking character of this consisted in its colour which was a bright yellowish green. With this there was mixed more or less red, and a few portions nearly white. The axillary glands formed a part of the general mass, from which they could not be distinguished by their colour or any other circumstance. The skin over the swelling had its corion very much thickened, and this change extended beyond the middle line of the chest. The tumor in most of its extent was distinct from the pectoral muscle, but had extended through it opposite to the ulcerated portion, and seemed to have converted the fibres completely into its own substance. The cellular tissue in both the pectoral muscles and in the intercostals, as well as in all parts about the region occupied by the tu-

mor, had the same remarkable green colour as the swelling itself. This colour extended along the subcutaneous stratum to the opposite side of the chest, and was observed in many points in the substance of the left pectoralis major. A very large mass of substance entirely resembling the original tumor was placed above the right clavicle, and extended backwards to the spine, involving the axillary artery and vein, and plexus of nerves. The right pleura was greatly thickened, particularly where it covers the diaphragm, and converted into a tough green substance; its cavity contained more than two quarts of a very bright lemon-coloured fluid. The lung was reduced to a very small size, and its covering of pleura was thickened; but there was no disease in its substance.

The peritoneum lining the right side of the diaphragm was thickened and green; and the membrane had undergone a similar change in two or three patches behind the abdominal muscles.

Two hard lumps, consisting of the substance of the gland indurated and turned green, were found in the pancreas; and a very considerable number of masses of the peculiar green substance were dispersed through both kidneys, the intermediate portions of these organs being quite sound.

A portion of cellular substance behind one of the vesiculæ seminales was hardened, and exhibited the green colour.

The lymphatic glands were very extensively diseased: they were enlarged in various degrees, and their substance more or less extensively converted into a hardish homogeneous mass of the colour already described. In some instances this deposition occupied the centre of the gland, and was surrounded by a thicker or thinner stratum of substance resembling the natural structure of the part; in some this external layer had a deep red and bloody appearance; other glands were converted entirely into the green structure. The cellular texture surrounding the glands was loose and natural. The axillary glands, on the diseased side, could not be distinguished from the original tumor, and those above the clavicle formed a large swelling of exactly similar appearance. From this a chain of enlarged glands extended to the trachea, and along its right branch to the lung, all the glands of which were affected. The axillary glands of the left side were very considerably enlarged. The right inguinal glands were slightly diseased: those situated about the external and internal iliac vessels, the aorta, and vena cava, as high as the diaphragm, were greatly enlarged, and formed one continuous chain. The largest of these were equal in size to hen's eggs, and had the bloody appearance externally. Two or three glands in the mesentery, one on the stomach, one behind the right rectus abdominis, and several in the posterior mediastinum were slightly enlarged, and partially converted into the green substance.

The large lymphatic trunks passing between those abdominal glands, which exhibited the bloody colour in the most remarkable degree, were distended with a reddish fluid. The thoracic duct contained a similar fluid, and was pervious throughout. The right absorbing trunk was pervious at its termination. Neither the left arm nor the lower limbs were at all œdematous.

The lining of the pulmonary artery and its branches, and of the aorta, together with its primary ramifications, was of a bright red colour, but not thickened.

The mucous membrane of the trachea and its branches, presented a deep red colour, and the surface was crowded with small vessels.

No part of the brain shewed any trace of disease; and the right axillary nerves, which were involved in the diseased glands and the primary tumor, were equally free from all morbid appearance.

CASE II.—A woman, who had laboured for some time under a singular affection of the arm, grew gradually worse, had a difficulty of breathing and general debility, and died. The subcutaneous tissue of the arm, from the elbow upwards, was converted on the inner side, and over about one half of the limb, into a solid brawny mass of about

an inch in thickness, which elevated the integuments into several small tubercles. Two deep ulcers, with foul surface, irregular everted edges, and unhealthy discharge, had formed in this diseased mass. The axillary glands, and those above the clavicle, were changed into a large tuberculated substance, resembling, in its hardness and very light brown colour, the original disease. Small depositions of a similar kind had taken place in the cellular substance of the biceps muscle; and several small portions, hardly producing any elevation of the surface, were found in the median nerve, for about two or three inches of its course. Very numerous tubercles were scattered over the surface, and through the substance of the lungs: they possessed a whitish colour, and firm homogeneous texture. They were most abundant on the surface, and varied in size from a millet seed to that of the end of the finger. There was no appearance of suppuration in any of them. The bronchial glands were enlarged.

CASE III.—In a patient, who had been for some time under medical treatment, on account of disordered intestinal functions, and whose breathing was affected only a few days before his death, a great number of very large and peculiar tubercles was found in the lungs. They were very firm, of a white colour, and nearly uniform in appearance throughout. Some were entirely imbedded in the substance of the lung, while others appeared on

the surface. They varied from the size of the fist to that of a nut. Nothing like suppuration appeared in any of them; but there was a slight blackish appearance in some, as if the substance of the lung had been imperfectly converted into the tumor. In the intervals of the tubercles the lung was healthy. The bronchial glands were considerably diseased.

Four or five small tubercles were observed on the heart, under its serous membrane.

A considerable tumor was found on the upper surface of the diaphragm, with the lung adhering to it; and a similar one was seen under the thick edge of the liver.

Five or six round and hard tubercles of different sizes appeared in the omentum; and many smaller ones under the peritoneum in various parts of the abdomen.

A white firm tubercle of the size of a nut, was found in one of the kidnies.

CASE IV.—In a woman, with whose history I am entirely unacquainted, the pelvis presented a most destructive disease. The neck of the uterus was destroyed; the remainder of that organ, the back of the bladder, and the front of the rectum formed the sides of a large cavity, with a rugged and

irregular surface covered by a horribly fetid discharge. The bladder communicated with this by a large opening, and the large intestine by two ulcerated apertures, one in the rectum, and the other in the sigmoid flexure of the colon. The fundus of the uterus had its natural appearance; but the part which contributed to the formation of the cavity, consisted of a white and softish matter. A similar substance surrounded the rectum, and connected it firmly to the sacrum; it extended into the substance of that bone, which it had rendered so soft that it could be cut with a knife. The inguinal glands of the right side were converted into a tumor of similar appearance. On the surface, and in the substance of the liver, there were several depositions of a white soft matter, of various sizes. The texture of the disease was very similar in all these parts. Maceration softened it considerably, and pressure then reduced it to a pulp, which would not have been distinguished from the substance of the brain.

DESCRIPTION

OF AN IMPROVED METHOD OF

TYING DISEASED TONSILS,

By THOMAS CHEVALIER, Esq.

SURGEON EXTRAORDINARY TO THE PRINCE REGENT, AND SURGEON TO THE WESTMINSTER GENERAL DISPENSARY.

Read February 18, 1812.

PASSING a ligature round a diseased tonsil, has generally been found an operation of considerable difficulty. The breadth of the tumor at its base, and the extreme impatience of the fauces, concur very much to embarrass the operator. I have lately been led to adopt a method, in which the operation is executed with great facility; and which completely succeeds, without the necessity of keeping an instrument in the mouth, after the ligature is applied.

I use a double ligature; and the improvement I am about to describe, consists, first, in the mode of passing that ligature; and secondly, in the mode of securing it when passed.

1. In order to pass the ligature, I use a flat, spear-pointed hook, fixed in a handle; the broadest part of which is about two lines. This hook is

passed behind the diseased tonsil, and its point is then pushed forward, so as to perforate it, through the middle of its base. It is then to be withdrawn. An eyed probe, very much carved, and armed with a long double ligature, may then readily be passed through the perforation, and brought out at the mouth, and the ligature divided, that one portion may be tied round the upper half of the tonsil, and the other round the lower.

2. In order to effect this, I have availed myself of a noose, much in use among packers; which, though it might readily be loosened, if a mechanical power were acting against it, remains under other circumstances perfectly fast; and is fully sufficient for the purpose in this, and other analogous cases, where a ligature is required.

A single knot being first made upon one end of the thread, the end so knotted, is to be brought forward upon the other, and to make a single noose upon itself including the other, and to be drawn tight upon it, close to the first knot: the free end of the thread is then to be passed through the ring of the instrument, as in Fig. 2. This end of the thread being then held firm, and the ring pushed forward upon the knot, the loop now formed, may be readily tightened, so as completely to strangulate the diseased part; and in the same manner it may be tightened from day to day, till the part is entirely detached.

The kind of thread I prefer, is thin, strong lay-cord: common packthread is so quickly rotted by the offensive saliva, which is afterward secreted, added to the warmth of the mouth, and the putrescency of the strangulated part, that in one instance in which I have employed it, it gave way, before the tumor was completely separated: the tonsil, however, was sufficiently grooved to make it readily included in a single ligature, which was passed without difficulty.

Instead of a simple, flat hook, I at first employed one with an eye, carrying the ligature; which, being passed through the base of the tonsil, might be seized with a hook, and brought forward: but I found that the combined irritation from the puncture and the thread in the fauces, was so great, that the most determined courage was scarcely equal to withstand it, for a sufficient length of time, to allow of the ligature being effectually laid hold on, and divided: and it was from being disappointed in attempting to execute this part of the operation in that manner, that I was first led to the more simple, and easy method of performing the operation, which I have now described.

A good deal of inflammation usually arises in the mouth and fauces, and about the sublingual and submaxillary glands, soon after the operation; which is best soothed by small doses of laudanum and antimonial wine. The mouth may be frequently

washed with warm water, and the poppy fomentation employed externally, if the pain should be considerable. This inflammation begins to subside on the third day. I think the tumor is generally detached on the fourth or fifth day, and the sore readily heals.

The ligature should of course be of sufficient length, to hang six or seven inches out of the mouth; and it will be an useful precaution to cover the ends with a piece of adhesive plaster, which may be fixed on any convenient part of the neck or face, to keep them out of the way.

Fig. 1. Represents the hook.

Fig. 2. Represents the noose before it is fastened, with the instrument upon the long end of the ligature, by which it is to be secured, after the noose has been tightened.

CASES

of

CYNANCHE LARYNGEA,

By J. R. FARRE, M. D.

Read Feb. 18, 1812.

CASE I.

On Monday evening, the 13th August, 1810, — Essex, aged 38, complained of sore throat. On the 14th at noon, his respiration was affected, and in the evening it was loud and hoarse, and he was long in completing each inspiration. He could not sit still for many moments, but walked about the room in the greatest distress, often struggling for breath. Unable to lie down, he passed the ensuing night in the same restless and agitated state, with occasional delirium. On the 15th, the difficulty of his breathing was in no respect diminished. On the 16th, at four in the afternoon, a medical man observed the following symptoms: respiration performed with extreme difficulty; pallid face, protruding eyes; an anxiety difficult to be

expressed; the uvula large, œdematous, and speckled, as if about to ulcerate; tonsils not affected; pulse laborious and frequent. The treatment had been hitherto limited to the use of cathartics, and the application of blisters to the chest and throat. A momentary relief was now obtained by taking twenty-four ounces of blood from his arm. At seven he was under the operation of a preparation of tartarized antimony and squills, which had excited vomiting, and was then acting on his bowels. The blood which had been drawn exhibited the appearances which are usual under active inflammation. At nine o'clock, when sitting up, he expired.

Morbid appearances 24 hours after death.

The velum palati, pharynx, and a portion of the œsophagus with the larynx, trachea and bronchia were carefully detached. The pharynx and œsophagus being slit down posteriorly, the mucous membrane investing the epiglottis, rima glottidis and the posterior part of the larynx, was found to be œdematous to so great a degree, as to make it evident that suffocation had been produced by stricture of the glottis. The tumefaction ceased at the junction of the larynx and trachea. The internal mucous membrane of the larynx was red and thickened. The traces of inflammation were less apparent in the trachea and bronchia, the membrane being irregularly reddened. No coagulable lymph had been effused on any portion of this membrane; and even its mucus was not

changed. There was an appearance of slight congestion in the lungs; some adhesions of the right pleura existed, but these were not recent; the heart was natural, and the liquor pericardii slightly increased. Excepting an enlarged spleen, there was no disease of the abdominal viscera.

In this case the inflammation did not pass the stage of congestion in the mucous membrane of the trachea, and of serous effusion under the mucous membrane surrounding the glottis; but in the following, the inflammation went into the adhesive stage, and closed the glottis by the effusion of coagulable lymph.

CASE II.--A poor, but industrious and temperate man, about 60 years old, on Sunday the 31st of March 1811, was affected with a painful and difficult deglutition. On the following morning, April 1st, alarmed at his inability to swallow fluid, which, on the attempt being made, returned by his nose, he sent for Mr. Weston who found the tonsils inflamed, and disposed to ulcerate. A brisk purge was ordered. At ten o'clock, on the same evening, his respiration suddenly became difficult, when Mr. Weston immediately drew from his arm thirty-two ounces of blood, which proved to be very sizy. In the course of an hour I attended, and observed the character of his disease at this stage. Although the tumefaction of the tonsils was inconsiderable, deglutition was extremely painful, and very difficult:

respiration was performed with convulsive and long continued efforts: his voice was nearly inaudible, and he could only whisper. He answered my inquiry respecting the seat of his suffering, by putting his finger on the superior part of the thyroid cartilage. He felt no pain in the chest. All the muscles of respiration were thrown into violent action, and he lay with his mouth widely opened, pupils dilated, face pale and sunken, skin covered with a clammy sweat, and his pulse at 133, and small. His powers were prostrate, and general bleeding could not be repeated. The anterior part of the throat was covered with leeches, but the disease never paused. At midnight, the operation of bronchotomy seemed to be the only resource; and soon after one o'clock, I decided on its employment.

About two o'clock, Mr. Astley Cooper attended, and as suffocation was instantly impending, the operation was immediately performed, by dividing, laterally, the ligament which connects the thyroid with the cricoid cartilage*. The dyspnœa was

* The following note, made by Mr. Cooper on the morning after the operation, he obligingly sent me with the drawing.

"I was called by Dr. Farre and Mr. Weston, at half past one A. M. on the 2nd April 1811, to a person aged more than 60, who had Angina Pharyngea. He was gasping for breath; every muscle that could be brought into action to assist respiration was employed. He was seized with sore throat yesterday, and at ten o'clock to-night with dyspnœa, which has increased so rapidly that he appears to be dying, pulse quick, face and lips pale, pupils dilated.

"I began

much relieved by the operation. He now lay passive, breathing by the natural and artificial apertures, and the inordinate action of the muscles of respiration had ceased. He swallowed some nourishment with a painful effort. In this state he passed the night, and the greater part of the following day. In the afternoon, the respiration by the natural passage entirely ceased, but was continued by the artificial aperture. He was now evidently sinking, and expired at six in the evening.

April 3d, eight o'clock A. M. Dissection by Mr. Astley Cooper.

The right tonsil inflamed and vesicated. The epiglottis swollen, its edges meeting behind, excepting just at the upper part. Pharynx inflamed, somewhat vesicated, covered with coagulable lymph about the epiglottis, but free from inflammation near its termination in the œsophagus. The aperture which had been made between the cartilages, appeared to be about half the size of the glottis.

The mucous membrane of the larynx and trachea was pale.

" I began the operation by making a cut between the thyroid and cricoid cartilage, longitudinally, and then transversely into the membrane which united these cartilages. No vessel of the least importance was divided, and he breathed through the opening, so as to be immediately relieved."

There was some accumulation of mucus in the cells of the lungs, and a slight effusion of serum into their reticular texture. The left pleura partially adhered, and the cavities contained rather more fluid than is natural to them. The abdominal viscera were not examined.

I may be allowed to remark that Cullen's character of his 4th species of cynanche will not embrace the cases which I have above described.

" Cynanche (Pharyngea) cum rubore in imis præsertim faucibus; deglutitione maxime difficili, dolentissima, *respiratione satis commoda*, et febre synocha."

The anterior part of the pharynx was indeed the seat of the inflammation; but that portion of the membrane being also common to the larynx, it may, in a practical sense, be useful to term the disease *laryngea*, because it proves fatal by constricting or actually closing the glottis, and constitutes precisely that case which, in its ultimate degree, imperiously demands the operation of bronchotomy.

HISTORY

OF

A CASE

OF

ANÆSTHESIA,

By JOHN YELLOLY, M. D.

PHYSICIAN TO THE LONDON HOSPITAL.

Read March 31, 1812.

THE following case exhibits an example of the loss of sensation in the upper and lower extremities, independent of paralysis: and as some of the particulars are curious, and the occurrence of such a complaint is exceedingly rare, I have drawn up an account of it for the use of the Society.

I. S. aged 58 years, a native of Scotland, resided for many years in Jamaica as a planter; and with the exception of being now and then bilious (to use his own term) he enjoyed, during the whole of this period, very good health. About three years ago, after being much heated and fatigued in his attendance, as one of the Grand Jury in Kingston, in

very sultry weather, he went to bed with the window open. On awaking in the morning, he found his feet and ancles perfectly numb, but without any sensation of pain, and without the muscular power being at all affected. Soon afterwards, he felt a numbness, with a tingling pain in his little finger, such as occurs in a part which is said to be asleep; and, by degrees, finger after finger became affected, until the whole of both hands was in a considerable degree insensible. He never had any pain or giddiness in the head, nor was he ever affected with the dry colic, which so often gives rise to paralytic affections in the West Indies.

A physician of eminence in Jamaica confidently assured him, that his complaints originated from scurvy in some lurking form or other; but it does not appear that Mr. S. ever had the least appearance of cuticular affection, till about 12 months after the occurrence of the numbness, when some red pimples shewed themselves in his legs, on his lying down on the ground, after being much heated by walking up a steep hill.

He arrived in this country on the 20th of July last; and from the time of my first seeing him, (which was on the 25th of October) till now, he had been as nearly as possible in the same state, except that his appetite and digestive powers, which were then a good deal affected, have been for some time past much improved.

The following are the principal circumstances which I have had occasion to observe relative to this case.

The hands, up to the wrists, and the feet, half way up the leg, are perfectly insensible to any species of injury, as cutting, pinching, scratching, or burning. The insensibility, however, does not suddenly terminate; but it exists to a certain degree, nearly up to the elbow, and for some distance above the knee. He accidentally put one of his feet, some time ago, into boiling water, but was no otherwise aware of the high temperature, than by finding the whole surface a complete blister on removing it. No species of injury to a vesicated part, of either hands or feet, is felt by him. The extremities are insensible to electrical sparks taken in every variety of mode. The cubital nerve where it passes the elbow communicates the sensation produced by pressure or a blow only half-way down the fore-arm*. He perspires much in the left side, just above the hip, but nowhere else. He is generally rather chilly, and the extremities are cold,

* I have remarked in some cases of paralysis from Colica Pictorum, that this nerve has its sensibility very much diminished; and in the case of a bricklayer, in whom there was a total loss of power, and in a considerable degree of sensation in both of the fore-arms and hands, from sudden exposure to cold, after working in a very hot furnace, the cubital nerve was totally insensible to any degree of pressure or force applied to it. It is probable that a similar insensibility might have been found to exist in other nerves, had they been accessible.

except when he is in a comfortable room; and in this case they are of the usual temperature of the body. The thermometer however rises to 101° in the mouth. The hands are of somewhat a purple hue. If he wishes to ascertain the temperature of any body, he is under the necessity of putting it to his face or neck, or the upper part of his arm. His skin seems to be more than usually sensible to the effects of heat. On putting his hand, at the desire of a friend, into a pail of hot grains, which his friend assured him were not too hot, and to convince him of this, previously thrust his hand and arm into it, there was a very extensive vesication produced. His hands are never free from blisters, which he gets by inadvertently putting them too near the fire; and he has met with several severe burns, without being aware of it. No degree of pressure is felt by him; but a blow produces a slight degree of tingling. He has a general uneasy sensation in the extremities, which warmth rather relieves. His skin, in general, heals very readily, after being burnt or scalded in the most severe way; and there is no fever, nor is there, as far as he has been able to determine, any increased heat or throbbing during the process of healing. This seems to be also the case with some other parts of his body which are not altogether insensible; for a little time ago, he got burnt in the front of the patella, and though there was a considerable slough separated in consequence, he suffered no pain, and little inconvenience during the cure.

If the heat to which the insensible parts are exposed is moderate, vesication is not immediately produced. The part becomes red, and in a few minutes, but sometimes not till the lapse of an hour or two, is blistered. That a smaller than ordinary temperature produces vesication in this individual, seems to be ascertained by the circumstance, that exposure to the heat of a common fire gave rise to a blister in the knee, which was followed by the slough abovementioned, though the clothes which covered the part, were not at all injured. Immersion in water at 120° produces no change in the affected parts; but exposure to the same temperature at a common fire, speedily blisters. That degree of heat, is, as far as I can judge by myself, about the utmost which can be long borne by the hand, in ordinary circumstances, without pain.

Cold water and warm water of every temperature invariably appear lukewarm to him. Water at the freezing point produces no degree of sensation whatever; but when his hand is kept in it for some time, he feels a slight coldness at the end of the thumb. After his hand has been immersed in water of the common temperature, and was introduced into water of 32°, he had some sensation of warmth. The cold produced by means of a mixture of snow or ice, and salt, communicates no sensation except a slight chilliness in the thumb, and when this freezing mixture follows the use of water of the common temperature, it produces a slight degree

of warmth. A solid body produces no sensation, whatever its temperature may be.

The power of motion exists in the muscles of both hands and feet. With the former he can grasp pretty firmly; but in holding any thing he is apt to drop it, if his attention is at all called away. There has been of late a slight loss of substance in the hands.

He can lift a chair, but cannot raise himself from the ground, by taking hold of any thing placed above him. The susceptibility of impression, generally, as well as the muscular power, seem to be in this individual in some degree diminished.

The functions are natural, and the pulse regular and moderate *.

In the treatment of these symptoms, no advantage has been derived from any plan which I have myself adopted, or which has been employed by others. The warm bath, electricity, galvanism, tonics, the rhus toxicodendron, and mercury, given both with a view to its action on the bowels, and the system at large, have been equally unavailing; though it must be owned, that the patient has never shewn a sufficient degree of perseverance, in the prosecution of any means which have been adopted for his relief.

* In Venerem minime habilis est.

The possession of voluntary power over the muscles of the hands and feet, which do not derive their origin above the part where sensibility commenced, might seem to be incompatible with a total loss of sensibility in such muscles; and it might therefore be imagined, and with some appearance of truth, that the insensibility was confined to the integuments. From the observations which I had frequently made upon this case, I had no doubt, however, of the insensibility extending to the muscles themselves; but in order to ascertain this point experimentally, I requested Mr. Travers, demonstrator of anatomy at Guy's Hospital, when examining the case with me, to introduce a couching needle into the fleshy part of the thumb; having previously had the best surgical authority (with which Mr. Travers agreed) for believing, that such an injury could be inflicted without danger or material inconvenience. A lancet-shaped couching needle was accordingly thrust by him, to the depth of nine sixteenths of an inch, into the ball of the thumb, at the distance of an inch and quarter from the middle of the dorsum of the metacarpal bone. It touched the bone, and must have passed through the mass of short flexors of the thumb. Not the least degree of pain, or even of sensation, was produced by this puncture. The part was desired to be poulticed, and the wound appears to have healed by the first intention; for the poultice was considered by the patient to be quite unnecessary after the 2nd day, and he left London, to embark for the West

Indies, at Gravesend, in three or four days afterwards, without having experienced the smallest inconvenience from it. I heard from his friends, before he left the river, where he remained a few days, that he was in his usual health.

Most of the cases of anæsthesia which have been mentioned by authors, have occurred in paralysis; in which complaint the degrees in which motion and sensation are lost, are very various, and do not bear any constant or regular proportion to each other. Where the anæsthesia has been wholly, or nearly complete, it has generally been described as taking place in one side of the body, while loss of the power of motion has occurred in the other. An interesting case of this kind, is given in the second volume of the Transactions of this Society*. The principal circumstances relative to it, consisted in a weakness, and diminution of voluntary power in the left side of the body, attended with a slight degree of numbness in its upper extremity, and a total loss of sensibility in the left side of the head, and in the right side of the body, from the neck downwards. In this case, there were some of the perversions of sensation, which I have mentioned as occurring in the present, particularly as to the feeling produced by cold water, which invariably appeared to be lukewarm.

* History of a case of singular nervous affection, attended with anomalous morbid symptoms. Medico-Chirurgical Transactions, Vol. II. p. 215.

I have met with two instances very nearly resembling that which I have now laid before the Society*. The first occurs in the American Medical Repository, and is communicated in a letter from Dr. Samuel Brown, of Lexington in Kentucky, to Dr. Miller, one of the editors. It is dated June 30, 1799, and is as follows†.

"Mrs. M'L. of Baridstown, in Kentucky, aged about 40 years, has been deprived for more than two years, of the power of sensation in her hands and feet. She is quite insensible of the effects of cutting instruments, or of burning coals applied to them. In one instance, when she was employed in shaping a piece of wood with a knife, she incautiously turned her eyes on some other object, and cut off the end of the thumb of her left hand, without perceiving the smallest sense of pain. She cannot, from her sensations, discover the least difference between a hot and a cold iron, and has frequently burnt the skin and flesh to a considerable depth, by mistaking the one for the other. These wounds and burns heal without any uncommon difficulty. Notwithstanding this total loss of sensibility, she retains the power of motion in full perfection, and pursues her domestic employments

* These cases I have met with since the paper was read to the Society.

† Medical Repository and Review of American Publications on Medicine, Surgery, and the auxiliary branches of Philosophy, Vol. IV. p. 225.

without any remarkable inconvenience. All her animal and vital functions are in a natural healthful state, and her spirits are regular, nay even cheerful. She feels no inconvenience from her complaint, except a sense of fulness in the veins, which she ascribes to the slow circulation of blood in the extremities. As the sense of touch however is entirely lost, she finds it difficult to retain substances in her hands without looking at them, as it is by sight, chiefly, that she regulates the degree of muscular contraction necessary to their retention. On turning her eyes aside, she often drops glasses, plates, &c. which she holds in safety as long as she looks at them. A variety of remedies had been tried without any effect. I was desirous of witnessing the effects of electricity. Although it produced very considerable contractions in the muscles of her arms, down to her wrists, the effects of it on her hands were scarcely perceptible. After two or three days however, she imagined that she was sensible of some kind of sensation from strong shocks, and was therefore advised to continue the application of it. Volatile liniment was prescribed, and rollers to support the veins which appeared relaxed and distended by their contents. Of the result of these experiments I have not yet been informed, as my patient lives more than 70 miles distant from Lexington."

The second case is given in a note in the Philo-

sophie Zoologique of Lamark. It is there mentioned, on the authority of M. Hebreard, that a man of 50 years of age, had for 14 years the right arm completely insensible. The limb, nevertheless, preserved its activity, size, and power. On the accidental occurrence of a phlegmon upon it, there were heat, swelling, and redness produced in the part, but no pain, even when it was pressed. During his work, the subject of this case happened to break the bones of his fore-arm, at about a third of their length from the wrist. As he only felt a crash (*craquement*) he thought he had broken the shovel which he held in his hand; but it was sound, and he could only discover his accident by being unable to continue his work. The following day, the arm, at the fractured part, was puffed up; the temperature of the fore-arm and hand was increased, but the patient experienced no degree of pain, even during the extension necessary to reduce the fracture*. In this case, it is clear, that the muscles of the affected arm were equally insensible with those of I. S. And it is probable that this was also the case with the person mentioned by Dr. Brown, though the evidence is not so decisive; as the injury done to the thumb might only have affected the integuments.

The existence of muscular power, and the faculty of directing its exercise by the will, where

* Philosophie Zoologique, par J. B. P. A. Lamark, Tom. 2. p. 262.—I have not been able to find the original case.

the nerves have entirely lost that sensibility which is always regarded as necessary to the conveyance of volition from the sensorium, are circumstances apparently irreconcileable with any knowledge which we at present possess, of the mechanism by which the will acts in the production of voluntary motion.

ACCOUNT OF

A CASE

OF

SPONTANEOUS EXTRAVASATION

WITHIN THE

THECA VERTEBRALIS,

WHICH SOON

TERMINATED FATALLY.

By THOMAS CHEVALIER, Esq.

SURGEON EXTRAORDINARY TO THE PRINCE REGENT, AND SURGEON TO THE WESTMINSTER GENERAL DISPENSARY.

Read April 14, 1812.

MISS D———, aged 14, for several days felt pain in the head and back, which induced her, on the 26th of February, to apply for medical assistance. Blisters were applied behind the ears, and opening medicines were ordered, which operated the following day, and relieved the pain in the head; but the pain in the back grew much worse, and was aggravated and accompanied with a tendency to sickness, on sitting up. On the 3rd of March the pain in the back increased much, and on the following day her mother thought she discovered

a little projection of the spinous processes of the lumbar vertebræ, in consequence of which I was desired to see her. I could not discover any thing in the appearance of the back that was not natural, nor was any particular pain excited by pressure. The effort to sit up distressed her much; but her countenance was clear and healthy. The tongue was rather white: she had a moderate, regular pulse at 120. Under these circumstances the pain did not excite any particular alarm in my mind: I directed some leeches to be applied as nearly as possible to the seat of the pain, and afterwards an opiate lotion: she was also ordered to take three grains of antimonial powder, with a saline draught every six hours, and I left her in full expectation of finding her relieved on the morrow, especially as, on strict enquiry, she could recollect no exertion or other violence by which the symptoms could have been produced.

On the following morning, to my great surprise, I was informed that, on the evening after I had seen her, she complained of a sudden and violent increase of the pain, and immediately went into convulsions, in which she continued between five and six hours and then expired. On the 6th of March I examined the body in the presence of Mr. Johnston of Mortimer Street, who had attended her from nearly the commencement. There was not the least appearance of disease in the brain or in any of the thoracic or abdominal viscera. The appear-

ance of the spine was also perfectly natural. I therefore proceeded to cut away the bodies of the lumbar vertebræ in order to expose the cavity of the spinal canal, close to the seat of the pain. I found it filled with extravasated blood, which from its florid colour must certainly have been arterial, and which covered the whole of the cauda equina. The cavity appeared to be filled much higher up; but I did not prosecute the dissection as it did not appear to me of much consequence to ascertain the exact limits of the extravasation, and the violence that must have been committed in opening the cavity farther, would have rendered it difficult, if not impossible, to tell precisely from what vessel the blood had issued. The original seat of the pain would naturally lead me to suppose that the rupture must have taken place near the commencement of the cauda equina, and it is probable the effusion of blood stopped within that part of the cavity inclosed by the dorsal vertebræ, as there was no appearance of it at the foramen magnum occipitale when the brain was examined.

The blood, though florid, was not all of an equal tint, it was very imperfectly coagulated; it is therefore probable that the vessel first gave way at the commencement of the illness, and again to a much greater extent at the time the convulsions came on.

I apprehend cases exactly of this description have not been very commonly seen; but that similar mischief should frequently occur, in consequence of

violent strains, is naturally to be expected: and I believe the foundation of carious spine, which is often laid by such accidents, is not unfrequently attended by some such mischief, which, as it may take place in various degrees, would account for the great disproportion one sees in examining a multitude of cases of this sort, between the degree of curvature in the spine, and diminution of power in the lower extremities.

A child of 12 months old, who had just recovered from the operation for hair lip, was carried out by the nurse. On its return home, it seemed in much pain, and appeared to have lost the use of its lower extremities: it died in three days. On opening the body I found the spinal canal full of a bloody serum, which, I have no doubt, was occasioned by slight extravasation from a strain and subsequent inflammatory effusion. And how destructive such extravasation, and inflammation combined may be to the structure and office of the nerves, was illustrated by the case of a miller, who suddenly lost the use of his lower extremities, by lifting a heavy sack of flour. He died on the 15th day after the accident. On examining the vertebral canal, some extravasated blood was found mixed with a sanious matter, the theca vertebralis was evidently inflamed, and the nerves of the cauda equina more completely rotten, than I have found them after many weeks' maceration in putrid water, after removal from the dead body.

These cases seem to have a very close resemblance to apoplexy, and point out the propriety of bleeding as early as possible after their occurrence, in order to prevent an increase of extravasation before constitutional debility takes place, and shew also the importance of distinguishing betwixt that primary weakness of the lower extremities, which is the direct result of the local injury, and that subsequent weakness in which indeed they will participate, but which is the effect of the shock the constitution has received, and which probably does not fully predominate till several days after; the intermediate time being occupied by more or less of symptomatic fever, which requires a moderate antiphlogistic plan of treatment.

OBSERVATIONS

ON

DIABETES INSIPIDUS.

By JOHN BOSTOCK, M. D.

OF LIVERPOOL.

Read April 28, 1812.

DIABETES INSIPIDUS, when existing as an idiopathic disease, is of such rare occurrence, that I presume the following case, which appears to me to be entitled to this appellation, will not be uninteresting to the Society*.

* The rarity of the disease is very decidedly proved by this circumstance, that since the time of Willis, who first pointed out the sweetness of the urine, scarcely any distinctly detailed case of diabetes insipidus is on record. Cullen, with that degree of scepticism which is so characteristic of a philosophic mind, hesitates whether he should admit its existence, yet he informs us that he had seen one case in which the urine was not saccharine*. The two cases related in Dr. Rollo's work, in which a large quantity of watery urine was evacuated, were consequent to a local injury of the kidney, and therefore do not belong to this class†. Nor do I regard the case in Dr. Duncan's annals, as entitled to the appellation of diabetes; it was attended with profuse perspiration, and it may be inferred from the remarks, that the ap-

* First lines, 4. 85. Synop. 2. 246. † p. 224, 5.

Mrs. ———, aged 50, about four years ago was under my care for a violent and long continued attack of menorrhagia, by which she was much reduced in her strength, but which was finally removed, and was followed by a total cessation of the menstrual discharge. She considered herself well, although a great degree of weakness remained; she complained of being much fatigued after using the least exercise, and remarked that although her appetite was good, yet that her food never appeared to give her any support. The general weakness and the peculiar state of the stomach increased upon her, and at length became so marked, that I was again called upon to visit her. The symptoms which she described, immediately led me to suspect diabetes, and I found

petite was impaired *. The only reference made by Cullen is to Lister; but upon an examination of this author, I think it will be found, that he believed in the existence of diabetes insipidus, merely upon hypothetical grounds. What a contrast does the prudent caution of Cullen, present to the hasty decision of Sauvages, who peremptorily declares, that all the diabetic cases of the ancients were insipid, because they do not mention the sweetness of the urine, while it is admitted, that ever since the time of Willis, all the cases have been saccharine †. In referring to Lister, it may be proper to observe, that the opinion which has been brought forwards with a certain air of novelty, of the stomach and not the kidney being the primary seat of the disease, was implicitly declared and defended by this author a century ago ‡.

* Duncan's Ann. 1801. 390. 1802. 361. † Nos. Meth. 3. 384.
‡ Exerc. Med. p. 74.

that the quantity of urine passed was much greater than natural, and that the calls in the night were so frequent as materially to injure her rest. I learned that for some weeks she had suffered from an unusual degree of scurfiness of the head, so much so, that it appeared every morning as if the whole cuticle were removed, and if the hair was not daily cleaned, the part was affected with the most intolerable itching. The whole surface of the body felt dry, and particularly the palms of the hands, but I did not perceive any eruption, except on the hairy scalp, or its immediate vicinity. Although the skin was frequently hot, yet she assured me that she never perspired.

The average quantity of urine passed by this patient in 24 hours, was 5 quarts; an ounce measure of it, which I examined, weighed 492 grains: therefore the whole quantity evacuated would be exactly 10$\frac{1}{4}$lbs. Troy. It was pale and nearly transparent; it had a faint odour; it slightly reddened litmus; and the specimen upon which the following experiments were made, had the specific gravity of 1.034. The residuum, which was left by slow evaporation, in its physical properties did not seem to differ from the extract of healthy urine. It appeared, by a simple calculation, that she was discharging between nine and ten ounces of solid water in the 24 hours; this may be estimated at about 8$\frac{1}{4}$ ounces more than that evacuated in the healthy state, while the ex-

cess of water above the natural quantity would be about 7lbs. Troy*.

The constituents of the urine were then more particularly examined. A quantity of the extract was digested in alcohol, and when all was removed that the alcohol was capable of dissolving, the fluid was evaporated. The part left by the evaporation of the alcohol, was to the part which the alcohol did not dissolve in the proportion of 17 to 4;

* The weight of the entire urine in the healthy state, and the proportion of the solid contents to the watery part, can only be obtained by an average estimate, and that subject to great uncertainty, in consequence of the numerous circumstances which produce a variation both in the quantity and the quality of this excretion. Haller, with his accustomed diligence and accuracy, has collected a great body of authorities with respect to the quantity of the urine, from which he fixes it at 49 ounces daily. But in forming this estimate, he has not exercised that judicious discrimination for which he is generally so conspicuous; after stating the different quantities that are given by different writers, (which vary from 28 to 64 ounces) he takes the general average, regarding the number more than the weight of his authorities*. The observations which appear to me by much the most accurate, are those of Rye, who fixes the quantity at 40 ounces†. The proportions which have been assigned for the water, and the solid contents of the urine are as various as those respecting the entire quantity. Haller has collected different estimates, which make the water from $\frac{19}{20}$ to $\frac{49}{50}$‡. Mr. Cruickshanks informs us, that the extent varies from $\frac{1}{17}$ to $\frac{1}{36}$ in the healthy state§; perhaps we shall not be far from the truth, if we take the average at about $\frac{1}{30}$.

* El. Phys. 26. 4, 6.
† App. to Rogers on Epid. Diseases.
‡ El. Phys. 26. 3, 14.
§ Thomson's Fourcroy, 3. 315.

and as the former of these consisted principally of urea, it appeared to be in this case rather more than four times the weight of the saline substances. By referring to the quantities mentioned above, it will be found that this patient was discharging daily about 7½ ounces of urea, and nearly 2 of different saline substances. I could not discover any portion of sugar in the urea, nor did I observe any thing peculiar in the nature of the salts.

With respect to the proportion which the animal matter in the urine bears to its saline ingredients, the statements of different writers are completely at variance. MM. Fourcroy and Vauquelin say, that the salts form only $\frac{1}{20}$ of the weight of the extract*, while Mr. Cruickshanks informs us, that they amount to about $\frac{3}{4}$ of it, or, more exactly, that they are to each other as 315 to 220†. It is not impossible, or even improbable, that the urine may differ from itself so much in different states of the body, as that both these accounts may be correct, as applying to extreme cases; but granting this to be the case, they still give us no idea of the average condition of the fluid. The results of two experiments, which I made with a good deal of attention, were that, in the first, the salts were to the urea as 13 to 69, and in the 2d, as 24 to 79; the average of these is as 1 to 4, a proportion which is not very far from the medium between

* Ann. Chim. 31. 68. † Rollo, p. 439.

the estimates of the French chemists and Mr. Cruickshanks. Supposing that the whole of the solid contents in the healthy state, amount to 600 grs. the urea will compose 450, and the salts 150 grs.; my patient was therefore discharging daily above 6 ounces of urea, and above $1\frac{1}{2}$ of salts more than the natural quantity. Such a waste of the system, may point out to us the final cause of the increased appetite for food, and the excessive thirst, which form some of the most characteristic symptoms of diabetes.

In endeavouring to detect the presence of sugar in this urine, I employed the action of nitric acid, and concluded that there was no sugar, because no oxalic acid was formed. In order to try how far this method of reasoning was correct, a quantity of pure urea, prepared from healthy urine, had $\frac{1}{10}$ of its weight of sugar added to it, and upon it was poured 6 times its weight of a mixture of equal parts of nitric acid and water, and heat was then applied. When it had been boiled to the consistence of a thick syrup, and was beginning to assume a dark colour, showing that the nitric acid was separated, it was removed from the lump; in a few hours crystals were formed, which, after being dissolved in water and recrystallized, seemed to consist of pure oxalic acid.

With respect to the treatment which was adopted in this case, I shall make only a very few re-

marks. As the urine was not saccharine, animal diet was not prescribed, and those remedies only employed which were supposed to have the power of strengthening the digestive organs, and reproducing a healthy state of the skin. For the former purpose the preparations of iron were chiefly relied on, and for the latter the warm bath; but I suspect that the morbid symptoms were so far connected, that whatever tended to relieve the one, was likewise beneficial to the other. The result was completely successful; the constitutional affections disappeared, the urine was reduced to the natural standard, and the surface regained its healthy action. The patient has remained well for above 12 months.

To the case that has been now detailed, I conceive the appellation of diabetes is strictly applicable; it possesses the three pathognomonic symptoms of the disease, the increased appetite for food, the suppression of the cutaneous discharge, and especially, the evacuation from the urinary organs of a larger quantity than ordinary of animal matter. This latter circumstance is essentially different from the mere increase in the quantity of water, a morbid state which has been often confounded with diabetes, but from which I regard it as totally dissimilar. The quantity of matter discharged from the urinary organs, when the disease is fully formed, or exists in an aggravated state, is so considerable, as to increase the specific gra-

vity of the fluid, notwithstanding the great increase in its quantity*. In this case it appeared, that the specimen upon which the experiments were made, which was obtained after the patient had been for some time without evacuating the bladder, had its specific gravity considerably increased, although the average specific gravity of the urine passed during the 24 hours, was not above the ordinary standard. Although cases of diabetes insipidus have been so seldom noticed, yet I am disposed to believe, that it is a more frequent occurrence than is generally imagined. It is seldom that the attention is directed with so much minuteness to the urinary discharge, as to enable the practitioner to ascertain the nature of its ingredients, and should the increase of its quantity be noticed, it is thought to be accounted for, by the increased quantity of fluids that are employed to allay the thirst.

We are perhaps not able to decide with perfect certainty, whether this disease be the commencement of the diabetes mellitus, or has a tendency to terminate in this state. I conceive, however, that the opinion is in itself probable, and it appears to be strongly confirmed by a case which occurred to Dr. Lewin, of Liverpool, the particulars of which he communicated to me, and upon which, at his request, I made some experiments.

* Henry in Medico-Chirurg. Trans. II. 119.

The patient was a middle aged female, and had a variety of complaints indicating general debility, and a decayed state of the digestive organs, which led him to suspect the existence of diabetes; and upon inquiry, he found that the urine was rather more copious than natural. He sent me a quantity for examination, the general results of which were as follows. It was somewhat opake, and of rather a browner colour than natural; by standing, it deposited a sediment, and then became more transparent. The different re-agents produced nearly the usual effects, except that the scaly crystals, formed by adding nitric acid to the urea, were less distinctly visible than ordinary. I thought the odour of the urea was less urinous than natural, but I was unable to procure from it any oxalic acid by heating it with nitric acid.

Dr. Lewin afterwards sent me a second specimen of urine, discharged by the same patient, the properties of which were, in several respects, different from what I had before examined. It was more opake, and deposited a greater quantity of sediment, its odour was sourish, and scarcely urinous. The extract produced by a slow evaporation, instead of being granulated, was of the consistence of a thick syrup, and when nitric acid was added, the scaly crystals were only imperfectly formed, and mixed with a quantity of uncrystallized matter. This half crystallized mass had a fresh quantity of nitric acid poured upon it,

and the heat of a lamp applied; a brisk effervescence was excited, and a vapor disengaged, which had the odour of nitro-muriatic acid. The process was continued until the fluid was nearly evaporated, and a mass was obtained, which, when cold, resembled candied honey. This was dissolved in water and partly evaporated, and in 24 hours some fine spicular crystals were formed, which produced a copious precipitate with lime water, and exhibited every property of oxalic acid. The result of this examination appeared to me so important, that I was desirous of having it confirmed by Dr. Henry, and I accordingly sent him a quantity of the solution of the honey-like substance. He replies to me as follows: "The smaller phial unquestionably contains oxalic acid, as I determined not merely by the fact of a precipitation, but by examining the precipitate, which, in all its characters, answered to oxalate of lime."

To Dr. Lewin's sagacity in detecting the nature of the disease in this early stage, we may consider ourselves as indebted for the important fact, that a small quantity of sugar can exist in urine along with a large quantity of urea*. The case is almost

* The fact here stated is precisely the converse of that advanced by Dr. Henry; he discovered that a small quantity of urea can exist in urine with a large quantity of sugar, while I have detected a small quantity of sugar united to a large quantity of urea. These facts may be considered as mutually illustrating each other, and become more interesting by their contrast.

equally decisive in favour of the conversion of the insipid, into the saccharine diabetes, and renders it probable that the two states alternate with each other, until, as the constitution becomes more and more impaired, the saccharine state of the urine predominates.

I have referred above to that morbid state of the urinary discharge, in which a large quantity of fluid is evacuated, but in which the animal matter exists in less proportion than natural. Such cases have generally been considered as diabetic; but I am disposed to doubt the propriety of the denomination, and to regard them either as the sequel of proper diabetes, or affections of a totally different nature, perhaps originating in a disease of the kidney*. A remarkable instance of a large discharge of watery urine, lately occurred to Dr. M'Cartney, one of the physicians to the Liverpool Infirmary, who sent me some of the fluid for examination. Upon pouring it from the bottle in which it was contained, it seemed so very unlike urine, that I concluded there had been some mistake respecting it, and requested that a fresh specimen might be procured. It appeared, however, that my suspicion was without foundation, for I received a second quantity, which exhibited exactly the same properties with the first.

* I gave an account of a case of this kind some years ago, which I called diabetes insipidus, but as I now conceive, incorrectly.—Mem. of Lond. Med. Soc. VI. 258.

It was nearly without colour and smell, slightly opake, neither acid nor alkaline, and had a specific gravity not much superior to that of water*. At the time when the patient was discharging $4\frac{1}{2}$ quarts in the 24 hours, the solid extract amounted to no more than $\frac{1}{170}$ part of the weight of the fluid. The action of nitric acid upon the extract produced a kind of spongy mass, in which the scaly crystals were scarcely, if at all, perceptible, nor was any oxalic acid formed when an additional quantity of nitric acid was heated with it. The oxymuriate of mercury threw down a slight flocculent precipitate, which was rendered more dense by boiling, but no effect was produced by heat alone, nor by the addition of tan. The other reagents, commonly employed to detect the salts of the urine, threw down precipitates, which seemed to be in the usual proportion to each other, but in smaller quantity than ordinary. Heated alcohol dissolved about $\frac{1}{3}$ of the extract; the alcohol, upon being evaporated, left a brown substance, which in its external appearance bore a resemblance to the urea, but which was found to differ from it in being reducible by a moderate heat to a dry state, when it assumed a kind of crystalline appearance. Nor did it produce any of the scaly matter with nitric acid. Upon the addition of this substance, there was a considerable effervescence, and the extract was converted into a spongy mass, that ap-

* Dr. Henry, to whom I sent a specimen of this urine, estimated the specific gravity at 1.0058.

peared to be composed of very minute cubical crystals, of nearly a white colour. That part of the extract, which was not soluble in alcohol, was chiefly dissolved by water; but a portion was left, which when separated and dried, had the appearance of a fine black carbonaceous powder: it composed about $\frac{1}{7}$ of the mass which was not soluble in alcohol. The composition of the extract would therefore be nearly as follows: 63 parts of a saline mass, which seemed to consist of the usual muriatic and phosphoric salts, 26 parts of an animal matter, and 11 parts of the carbonaceous powder. The urine soon acquired a nauseous smell, became slightly alkaline, and deposited a small quantity of a flaky sediment; but it afterwards remained for several months exposed to the atmosphere, without experiencing any farther change.

I relate this case rather as exhibiting a singular and curious state of the urinary discharge, than as directly leading to any physiological conclusions. One circumstance, however, is worthy of notice, that in this, as well as in the case of watery urine which I formerly published, the deficiency of the salts is not so considerable as of the animal matter. In this respect the urine seems to be analogous to the albuminous fluids of the body: for in the elaborate experiments of Dr. Marcet, which have been related to the Society, the salts were always found nearly in the same quantity, however various was the proportion of the animal matter; a circum-

stance which coincides with my own experiments on these substances.

Knotshole Bank, near Liverpool,
April 18, 1812.

Experiments on the Extract from Diabetic Urine.

THE following train of facts, although not immediately connected with the subject of the above paper, I consider not undeserving the notice of the Society, as tending to throw some light upon the nature of the extract procured from the urine of diabetes mellitus.

About eight years ago, I procured some diabetic extract from urine, which was so highly saccharine, that by mere evaporation at a low temperature, the whole was converted into a dry substance, of a granulated and half crystallized texture, which in its appearance very nearly resembled fine brown sugar. It remained for a long time without undergoing any alterations; but having been placed in a damp room during the last winter, upon examining it some weeks ago, I found its appearance entirely changed. The paper containing it, which consisted of several folds, was covered with a thick coating of mould, not unlike the mould of cheese. The substance that remained within the paper was much diminished in bulk, and had lost all appearance of sugar; it was of a viscid consistence, and

looked like half melted glue; it was of a brown colour and musty smell. It seemed to experience no farther change by exposure to the atmosphere. Water, when boiled with it, was tinged of a light brown colour; the substance seemed to be rendered whiter, but it did not appear to be soluble in this fluid. Boiling alcohol rendered the substance harder and whiter, but no solution was effected. Caustic potash, when assisted by heat, dissolved it in moderate quantity; the solution was brown, soapy in its consistence, and frothed much when boiled; sulphuric acid threw down a white flaky precipitate from the solution. Nitric acid, when diluted with an equal bulk of water, soon began to act upon this substance; when heated to ebullition the substance was dissolved, with the disengagement, first of nitric vapor, afterwards of nitrous gas. The colour of the acid was much deepened, and its consistence was thickened; as it cooled, some flakes separated that resembled a concrete oil: the acid had acquired that peculiar odour which attends the action of nitric acid upon the muscular fibre. When potash was added in excess, the nitric solution acquired a deep orange colour, a grey precipitate was thrown down, and the fluid was left nearly transparent. When ammonia was added to the nitric solution, there was a very copious emission of white fumes, and the same change of colour as with potash.

These experiments were sufficient to show, that

the saccharine extract had been converted into a substance very nearly resembling coagulated albumen. It is probable that the albumen must have existed in the first instance, mixed with the saccharine matter; but in what way the saccharine matter was decomposed, and the albumen coagulated, are points that I confess myself not able to determine.

CASES

OF

PREMATURE LABOUR

ARTIFICIALLY INDUCED, IN

WOMEN WITH DISTORTED PELVIS:

TO WHICH ARE SUBJOINED

SOME OBSERVATIONS

ON THIS

METHOD OF PRACTICE.

By SAMUEL MERRIMAN, M. D.

PHYSICIAN-ACCOUCHEUR TO THE MIDDLESEX HOSPITAL, AND TO THE WESTMINSTER GENERAL DISPENSARY.

Read May 12, 1812.

MRS. POPE, 21 years of age, had been many hours in labour of her first child, under the care of a midwife, when my assistance was requested, on account of the difficulty of the case. When I arrived at the house, I was informed that the difficulty arose, from the heads of two children being in the pelvis together; but upon an examination *per vaginam*, I discovered, that the projection of the sacrum had been mistaken by the midwife for the head of a second child. As the distortion of the pelvis was so great, it was evident, that the labour could not be terminated safely to the mother, un-

less the child's head were diminished. I resolved, therefore, to have recourse to the perforator; but as the child's head lay very high, and the os uteri was by no means sufficiently dilated, it was necessary to wait many hours, before I could proceed to the operation. At the end of eighteen hours, the os uteri being more dilated, and the head of the child being forced lower, so as to be more within the reach of the perforator, I made an aperture through the cranium, and extracted the parietal bones. Notwithstanding this diminution of the head, it required a considerable length of time, and very strong pains, to accomplish the extraction of the child. The mother recovered slowly, but at length perfectly, from the effects of this labour. This took place in September, 1804.

In August, 1807, I was again called to her assistance, when in labour at the full time; and was obliged to effectuate the delivery, by the same operation as before. I now very strongly recommended to her, to have her labour excited at seven months, in any future pregnancy; this offering the only chance, of her having a live child, and my uncle, Dr. Merriman, who gave me his assistance, both upon this and the former occasion of my attending her, joined with me in this recommendation.

In a few months, she became pregnant again, and having fully completed the seventh month of

gestation, and being in good health, I punctured the membranes, in the evening of September 26th, 1808, giving her afterwards a dose of laudanum. Labour pains came on, in about twenty-four hours after the operation, and I was called to her, early in the morning of the 28th, when I found the *nates* presenting. During the progress of the labour, the child was felt strong and lively; but before the head could be brought through the narrow part of the pelvis, the pressure on the navel string was so great, as entirely to stop the circulation, and thereby occasion the child's death.

In November, 1811, having, according to her reckoning, completed seven months and a fortnight of her fourth pregnancy, I punctured the membranes, on Thursday the 7th, in the evening. The waters were discharged in a very gradual manner; and on Tuesday the 12th, when labour pains came on, the feet were found to be the presenting parts. In consequence of this mal-presentation, the child perished from the compression of the navel string; for it was not possible, to bring the head of the child through the narrow part of the pelvis, with sufficient expedition, to preserve its life.

After both these deliveries, the woman recovered remarkably well.

Mrs. North, 26 years of age, when advanced to the seventh month of her first pregnancy, met with

a fall, or some other accident, which threw her into premature labour. I was informed, that upon this occasion, her delivery was effected without difficulty, and the child was born, with so much of life, as to cry faintly once or twice, before it expired.

In November, 1809, she fell into labour of her second child, at the full period of gestation. It was now for the first time discovered, that her pelvis was very much deformed, and that there was no possibility of the delivery being effected, without extraordinary assistance. The surgeon-accoucheur in attendance therefore requested, that there should be a consultation upon the measures to be adopted; and two physician-accoucheurs being joined in consultation with him, it was judged absolutely necessary, to have immediate resource to the perforator. Accordingly the contents of the cranium were evacuated, but it was not till after a very long, hard labour, that the child could be brought into the world.

In her third pregnancy, she consulted with Dr. Denman, and myself, respecting the expediency of having premature labour brought on. As we both agreed in opinion, that this would give the only chance of her being delivered of a living child, it was determined to perform the operation. Having, as she believed, completed seven months and a fortnight of her pregnancy, and being in good

health, I punctured the membranes, in the evening of Tuesday, February 13, 1811. The pains of labour began, in the night of Thursday the 15th, and I was sent for the next morning, when I had the mortification of finding the child's arm in the vagina.

Having apprized Dr. Denman of this untoward position of the child, he came immediately to see the patient, and to afford his advice and assistance. The os uteri appearing to be dilateable, I cautiously endeavoured to introduce my hand, for the purpose of turning the child; but the irritation this produced, threw the uterus into such strong action, that we thought it more prudent to desist, and to allow some more hours to pass over, before another attempt should be made. We expected, that, in the mean time, the parts would become more dilated, and as the child was small, that the feet would be forced by the pains, nearer to the os uteri, by which the operation of turning would be much facilitated.

At 8 o'clock in the evening, I made another attempt to turn the child, and accomplished it, without more difficulty than usually occurs in turning, in a contracted uterus, after the liquor amnii has been so many hours evacuated.

The feet, being brought into the vagina, were soon expelled by a pain, and almost immediately

afterwards the hips protruded through the os externum. At this time, the pulsation in the funis was strong, and the child vigorous; but when the head became wedged in the narrow part of the pelvis, the navel string was so much compressed, as soon to occasion the child's death. It was nearly half an hour before the head could be brought into the world, though the action of the uterus continued very strong, at intervals, during the whole time.

Immediately after delivery, the mother was seized with a very violent shivering fit, of more than an hour's duration. This was followed by so great a degree of fever and delirium, as to create considerable alarm for her safety; and I was, in consequence, induced to order for her, an active cathartic, preceded by a dose of calomel, in a very few hours after delivery. From these remedies, a copious evacuation of very offensive, dark coloured fœces was procured, which gave her great relief. Indeed it is probable, that the early exhibition of these purgatives saved her life, for her fever ran very high, and she continued in a very dangerous state, for seven or eight days. At length however she perfectly recovered.

Mrs. Jenkins, 22 years of age, in labour of her first child, was attended by a very experienced midwife. Her labour began on the 15th of November, 1808. During the first twenty-four hours, the pains were frequent but slight, the liquor amnii was then

evacuated spontaneously, and the pains became very strong and frequent, but the child's head made little progress, in descending into the pelvis. My assistance was therefore requested on the 17th in the evening. This woman had been extremely ricketty when a child, and the pelvis was in consequence very much deformed.

I believed it necessary to have recourse to the perforator without delay, and having removed one parietal bone, I left her under the care of the midwife, that the pains might force the head, thus diminished, lower into the pelvis. At the end of five or six hours, finding that no apparent progress was made in the labour, I endeavoured to extract the head, by the help of the blunt hook, but could not finish the delivery without great difficulty.

At the beginning of the present year (1812), I learnt, that this poor woman was again pregnant; and being convinced by an examination, to which, at my request, she now again submitted, that it would be impossible to deliver her of a living child at the full period of utero-gestation, I determined with her ready consent on performing the operation for inducing premature labour. Accordingly on Wednesday, March 4, 1812, I punctured the membranes in the evening, giving her afterwards a dose of tinct. opii. Labour pains came on, Monday, March 9th, and she was delivered of a live child on Tuesday, 10th, after about eighteen hours of very strong la-

bour. The projection of the sacrum occupied so much space in the pelvis, as to produce a very marked depression of the right parietal bone, which did not recover its shape for four or five days.

At the time the operation was performed, Mrs. Jenkins reckoned that she had advanced about nine or ten days beyond the seventh month of gestation; so that the child, at its birth, must have been within a fortnight of eight months' growth. It is at present a thriving child; and there is great reason to believe, that it will be reared to maturity.

The practice of inducing premature labour, in cases of distorted pelvis, was first adopted in London, as is well known, about the year 1756*; when there was held a consultation of the most eminent practitioners of midwifery, "to consider of the moral rectitude of, and the advantages that might be derived from, the practice, which met with their general approbation†."

The morality of the practice, though it has

* The introduction of this method of practice has been erroneously attributed to Mr. Barlow, of Blackburn, in Lancashire, who published several cases of the issue of this practice, in 1800. It had been mentioned, by authors and lecturers on midwifery long before his paper was published.

† Denman's Introduction to Midwifery, 4to. p. 395.

been much questioned since this time, does not appear to have been doubted by these experienced practitioners. For the proposal was, that labour should be brought on at seven months, in those cases *only*, where it had been previously ascertained, that the pelvis was too much contracted, to allow a full sized fœtus to pass undiminished. By this operation, therefore, it was proposed to give a chance to the mother of bearing a living child at seven months, which must of necessity be sacrificed to her safety at nine months. The idea was probably suggested, by the success which had occasionally attended the practice, recommended by Guillemeau and others, of rupturing the membranes at an early period, in cases of profuse uterine hæmorrhage, during pregnancy.

Objections to the morality of this practice, are much more frequent in the writings of foreign practitioners of midwifery, than in those of this country. Sue, Baudelocque, and Gardieu, all accoucheurs of great celebrity in France, decidedly object to it, not only because they think that the practice is contrary to the laws of nature, but because they say it is not to be depended upon, either for lessening the sufferings of the mother, or for saving the life of the child*. M. Gardieu

* Il y a déjà long temps que M. Petit a conseillé et fait pratiquer l'accouchement prématuré dans le cas de difformité aux os du bassin. Il a meme proposé à ce sujet des moyens, dans le détail desquels ce n'est pas ici le lieu d'entrer, avec d'autant plus

tells us, that it lays the foundation for cancers and other dreadful diseases of the womb, and thinks that a woman had better run the risk of the Cesarean operation, or the division of the symphysis pubis, than submit to this operation*. The objections of these celebrated French accoucheurs, are plainly the result of a theoretical investigation of this method, for they acknowledge that they have had little or no experience of it.

de raison, qu'il faudroit d'abord que les casuistes et les théologiens eussent décidé s'il est permis d'accélérer par art, une fonction à laquelle la nature a assigné un terme fixe, et si on peut courir les risques de la vie incertaine de l'enfant, pour assurer celle de la mere. Sue, Essais historiques sur l'Art des Accouchemens. T. 1. p. 606.

L'accouchement prématuré obtenu par de semblables moyens, est toujours si peu favorable à l'enfant, qu'il nous semble qu'on ne devroit le permettre que dans ces cas d'hemorrhagies abondantes, qui ne laissent d'espoir de salut pour la femme que dans sa delivrance.

L'accouchement prématuré, si on le sollicitoit constamment au même terme, pourroit être aussi long, aussi laborieux, et tout aussi infructueux, même aussi impossible en quelque cas, que si l'on n'eut enterpris de délivrer la femme qu'au neuvieme mois.

Baudelocque, Traité des Accouchemens, T. 2. p. 344.

* La femme ..... est atteinte d'une inflammation de la matrice, qui est une suite du travail contre nature, et des efforts necessaires pour ouvrir le col, qui ne se trouve pas disposé favorablement : si elle ne succombe pas aux accidens primitifs, elle est exposée par la suite aux squirres, aux ulceres, aux cancers de la matrice qui lui rende la vie à charge, et lui feroient préferer d'avoir respecté la grossesse, au risque de s'etre exposée aux dangers qu'auroient pu lui faire courir l'une des deux operations [L'operation Césarienne ou la section du pubis] qui devient nécessaire pour operer la delivrance dans ces rétrécissemens extrêmes.

Gardieu, Traité d'accouchemens, T. iii. p. 18.

The generality of writers admit the safety of the practice, as far as it regards the mother: perhaps the safety of the practice has been too generally admitted. Dr. Denman, in relating the particulars of a case which he attended with the late Dr. Savage, says, "on the following day, (after the evacuation of the liquor amnii,) she had a rigor, succeeded by heat and other symptoms of fever, which very much alarmed us for the event*." And a lady in the country upon whom this method was tried, was seized with severe shivering fits, followed by so much fever, as made her relations and medical attendants despair for some days of her recovery†. In the cases above related, one of the patients, Mrs. North, was for several days in a state of great peril. It may be alleged that the danger which she experienced, arose rather from the mal-position of the child, requiring it to be turned in utero, than from the premature excitement of labour; but I am more inclined to attribute the severe shivering fit, which attacked her, to the latter cause, because shivering seems to be no unusual consequence of this operation.

Whether the death of the mother has ever occurred from this method of practice, I am unable to state positively. I have heard of three women who died in a few days after delivery thus artificially

* Introduction to the Practice of Midwifery, 4to. p. 398.

† Communicated to me by the lady's sister.

induced, and their deaths were attributed to the operation; but whether justly or not, I am not sufficiently acquainted with all the circumstances to judge. At all events, the method in question, if carefully conducted, cannot be more hazardous to the mother, perhaps is much less so, than the operation for lessening the head of the fœtus in utero, and it is incomparably less perilous than the Cesarean operation, or the division of the symphysis pubis.

The best defence of the practice, however, is derived from the chance it affords of preserving the life of the child. In the greater number of instances, indeed, the child will either be dead born, or will be born with so little life as to expire in a few hours; but in many cases the child has been preserved.

Dr. Denman, in his Introduction to the Practice of Midwifery, reports *twelve* cases of this operation, and adds, that the majority of the children were born alive, but does not mention the exact proportion.

Mr. Barlow, a surgeon of great respectability and celebrity at Blackburn in Lancashire, published in the 8th volume of the "Medical Facts and Observations," *seventeen* cases. Six of the children were dead born, and *five* others, though born with

life, died in a few hours; but *six* were born alive and capable of living*. In the 4th Volume of the "London Medical Review and Magazine" for 1800, were reported *ten* cases of this operation, with which I became acquainted, from their having occurred in the practice of my uncle, Dr. Merriman. *Four* of these children were born living, and likely to live. At the commencement of this paper, I have detailed *one* successful, and *three* unsuccessful cases, and I have further to report, from the information of my friend, Mr. Marshall, surgeon, of Half-Moon Street, Piccadilly, *four* other cases, *one* of which was completely successful. Thus out of *forty-seven* instances of distorted pelvis, in which this operation had been practised, at least *nineteen* children had been born alive, and capable of living.

If it be considered, that in all these cases, the degree of distortion of the pelvis was so great, as to preclude the possibility of a fœtus at nine

* Mr. Barlow has described the time when the operation should be performed, rather inaccurately. He says, "my method consists in exciting premature labour, *early in the seventh month* of pregnancy; but he should have said *early in the eighth month*, that is after the *after the seventh month is completed*. *Early* in the seventh month, implies a time soon after the *sixth month is completed*, at which time there is no chance of the child surviving, even though it should be born alive. That Mr. Barlow meant for the operation to be performed at seven complete months, is apparent from his afterwards saying, that "the child at *seven months* old has a sufficient chance of surviving the birth."

months of gestation passing alive, or undiminished, there is, I presume, sufficient proof that the practice is not only admissible, but that it ought to be recommended in such deplorable cases of distorted pelvis.

In most of the unsuccessful cases, the cause of the child's death was either not at all adverted to, or has not been made known; but this is a point that deserves attention. In many instances the death of the child seems to have taken place immediately after the severe rigors and fever, already alluded to, which attacked the mothers within a day or two after the liquor amnii was evacuated. It is possible that by proper management after the operation, these unfavorable symptoms may occasionally be averted. It appears likewise, that a preternatural presentation of the fœtus was a frequent cause of the child's death.

Dr. Denman does not take notice of any case of cross birth, among the twelve that he has recorded, but among Mr. Barlow's cases, there are at least two of preternatural presentation: in *two* of the cases communicated to me by Mr. Marshall, and in *four* of those which have occurred within my own knowledge, the presentations were likewise preternatural:—in all these cases, the children were dead born, probably from this cause. The proportion of preternatural presentations, according to this estimate, is about one in six.

It may be proper to enquire, whether preternatural presentations are equally frequent, in labours which occur spontaneously before the proper period? But the determination of this question requires more data than I at present possess. I have however now before me, a list of *seventy-eight* labours occurring prematurely, either from the spontaneous action of the womb, or from accidental violence, and I find that among them, there were *seventeen* cases of preternatural presentation, viz.

7 of the nates,
5 of the feet,
3 of the arm,
1 of both hands,
1 of the funis,

but I should suspect that this gives a greater average of preternatural presentations, than commonly happens in premature labours occurring spontaneously*.

* Authors are much divided in opinion, respecting the proportions of preternatural presentations of the fœtus, at the full time of gestation. Smellie estimates the proportion at 1 in 100. Professor Camper, of Amsterdam, from the documents afforded by Messrs. Titzing and Beckman, reckons them at not more than 1 in 150; and Professor Jacobs, in his "Ecole pratique des Accouchemens," published at Ghent, in 1785, states the average of preternatural presentations at not more than 1 in 160. On the contrary, Dr. Bland, who has made the calculation with great exactness, from the registers of the Westminster General Dispensary, (Philos. Trans. Vol. LXXI.) states the average of preternatural presentations at 1 in 30; there having occurred in 1897 labours, 36 presentations of the nates,
18 ..... of the feet,

8 pre-

It has been a subject of inquiry, whether other circumstances, besides distortion of the pelvis, do not sometimes exist during pregnancy to make the inducement of premature labour expedient: as in cases of extreme weakness, or illness on the part of the mother, whose recovery seemed to be impossible if her gestation were suffered to continue; or in some of the more severe complaints peculiar to pregnancy, which were imagined to place the mother's life in imminent danger. An extension of the practice to such cases has been strongly re-

8 presentations of the arms,
1 ..... of the funis.

If the average given by Dr. Bland, is what usually occurs, other authors cannot have been sufficiently accurate in their calculations: perhaps they only mean to speak of such cases of preternatural presentation as require extraordinary assistance. That they have omitted to enumerate many nates and feet presentations, is rendered probable from the following fact. Messrs. Titzing and Beckman, state the number of arm presentations in 18 years at 98, of the nates at 66, of the feet at 61. But it is well known, that nates and feet presentations are much more frequent than those of the arm; so that it cannot be doubted that many of the former must have been overlooked.

The exactness of Dr. Bland's calculations has been strongly confirmed, by the return of preternatural presentations, which occurred in the "Maison d'accouchemens," at Paris, during a period of nearly ten years, to May 31st, 1809. From this it appears, that upon an average preternatural presentations happened once in 27½ labours: of these

296 were presentations of the nates,
215 ..... of the feet,
and only 60 ..... of the superior extremities.

The number of labours from which the average is drawn, is 17,499.

commended, by a provincial Surgeon of considerable eminence.

This gentleman says, " The preservation of the child is obviously the primary object for the bringing on of premature labour, in the distorted pelvis; yet if the safety of the mother, under particular circumstances without distortion, should require similar means to be employed, with safety to the child, surely no good reason can be assigned, why they ought not to be adopted."

If indeed it could be proved, that the safety of the mother required such a mode of practice, and that the safety of the child would not be implicated by the operation, the argument might have some weight, but it seems hasty to assume that such is the fact.

Even in the case which the author adduces in support of this recommendation, it may be doubted whether the safety of the mother required this method of procedure. She was indeed teased with a very severe cough, and her stomach was so irritable as to retain no food whatsoever, nor even opium in a solid form. She had taken absorbents, stomachics, bitters, aromatics, and opiates without experiencing any relief; liniments, fomentations, and blisters, had been externally applied without benefit, and she was thought to be sinking into her grave, when it was proposed, as a last resource, to

bring on premature labour, six weeks before the full time, and the patient was delivered of a living child, and ultimately recovered.

Men in extensive practice in midwifery must be aware, that the state of irritability of stomach here described, occasionally takes place at all stages of pregnancy; that however severe it may be, the complaint frequently ceases before the termination of the pregnancy, and that very small quantities of simple diet, containing but little nourishment, and nothing stimulating, will, under such circumstances, support the mother for an inconceivably long space of time, without destroying, or injuring the fœtus. It may likewise be remarked, that this state of irritability of stomach is generally connected with more or less of inflammatory action, either in that viscus, or in some of the neighbouring parts; to relieve which, bleeding is perhaps, of all others, the most appropriate remedy: while cordials, and aromatics serve but to aggravate the complaint. Now as the patient in question took remedies of the latter class, while the former appears to have been neglected, I think this case cannot be held up, as a proof of the necessity of resorting to this expedient.

Writers of the best authority have remarked, that in acute and dangerous diseases, the spontaneous occurrence of abortion or premature labour adds greatly to the danger of the patient. This is a

strong reason against exciting premature labour by art, during the continuance of such diseases. And though the practice of inducing premature labour in cases of distorted pelvis has rarely been attended with alarming consequences, yet I apprehend that I have mentioned enough to shew, that there is no absolute freedom from hazard, even when the woman's health may be called good at the time of the operation, and how much the degree of hazard must be increased, when the patient is already in a state of great danger, need not be insisted upon.

As to the safety of the practice with regard to the child, that likewise seems to be assumed upon no very substantial grounds; at least if we may judge of the probability of saving the child, from what has happened in cases where distortion existed, we shall not have much encouragement, so far as the child is concerned, to extend the practice to such cases. If indeed the majority of the children had been preserved, something might be said in support of the operation; but it seems, that only *nineteen* children were born alive, and *capable of living*, out of *forty-seven* instances of the experiment.

These are indeed sufficient to justify the practice, when there is a moral certainty that the child's life must be inevitably lost, unless this mode be adopted; but there does not exist the same reason for resorting to it, when the child's life is not so certainly exposed to danger.

6. The utmost care should be taken, to guard against the attack of shivering and fever, which seems to be no unusual consequence of this attempt to induce uterine action, and has often proved destructive to the child, as well as alarming with regard to the mother. The peculiar circumstances under which the operation is performed, and the habit of body of the patient will determine the accoucheur either to adopt a strictly antiphlogistic plan, or to exhibit opiates, or antispasmodics and tonics.

7. In order to give every possible chance for preserving the life of the child, it will be prudent

been known to happen. Dr. Denman has known more than one instance of the presentation of the head being exchanged for the arm. Introd. to Midwifery, p. 450. Mr. Burns mentions a case, where the shoulder was exchanged for the head, Princip. of Midwifery, p. 218. Dr. Merriman, senior, was about to puncture the membranes in a woman who had a distorted pelvis, when he distinctly felt the child's hand through the membranes, just within the os uteri; he therefore deferred the operation, and at the end of three or four days, examined again, and discovered that the hand was retracted and that the head presented. He now performed the operation, which was completely successful. Silber tells of a case in which the elbow presented, but in thirty-six hours, the head occupied its place. "Pater meus explorans per vaginam Fœminæ adpariendum vicinæ, cubitum fœtus prolapsum, et per integumenta caput in dextro matris latere invenit: nihilominus tamen post horas triginta sex, aquis effluxis caput situ naturali ingressum est."—Silber de viribus naturæ medicatricibus in situs fœtuum iniquos Tubingæ 1799.

Other such cases are to be met with in authors.

to have a wet-nurse in readiness, that the child may have a plentiful supply of breast milk from the very hour of its birth.

Lastly, *a regard to his own character should determine the accoucheur, not to perform this operation, unless some other respectable practitioner has seen the patient, and has acknowledged that the operation is advisable.*

EXPERIMENTS

ON THE BARK OF THE

COCCOLOBA UVIFERA.

By JOHN BOSTOCK, M.D.
OF LIVERPOOL.

Read May 12, 1812.

ALTHOUGH kino has been, for a considerable time, well known as an article of the materia medica, there still remains, some uncertainty respecting its origin. It is indeed generally supposed, that there are three substances, somewhat different from each other, to which this name is applied. The one which has been the longest known, and is perhaps the most frequently met with, comes from Africa; but we are entirely ignorant from what plant it is derived. A second species is said to be extracted from the Eucalyptus resinifera of New South Wales; while a third variety, which is brought from the West Indies, has by some writers been ascribed to the coccoloba uvifera, and by others to the mahogany*. Having obtained a quantity of the bark of the coccoloba uvifera from a friend in

* Duncan's Ed. Disp. 292; Thomson's Lond. Disp. 213; Murray's Mat. Med. 2. 304; Nicholson's Journ. 6. 232.

the West Indies, on whose accuracy I could implicitly depend, I embraced the opportunity of examining its properties, and comparing the extract formed from it with the kino usually employed in medicine.

The bark appeared to have been taken from branches of from one to two inches in diameter, and was partially rolled up, much after the manner of the common Peruvian bark. No part of it was more than $\frac{1}{16}$th of an inch in thickness, and some specimens considerably thinner. It was lined with a fine reddish brown cuticle, while the rest of the bark was of a light yellowish brown colour. The external surface was clean, and nearly free from any protuberances, but in many parts it was marked with slight longitudinal furrows. When cut transversely, the external part of the bark, for about one quarter of its thickness, exhibited a coarser texture than the remainder, and could not be reduced to an equally fine powder. It had scarcely any smell; when chewed and kept for some time in the mouth, it produced a moderate degree of bitterness and astringency, with a slight mixture of an aromatic flavor.

A portion of the bark reduced to fine powder, from which a little of the coarser part had been separated, was mixed with 40 times its weight of water, and kept for an hour at the heat of 200°. A light brown fluid was formed, which was filtered

while warm. Athough at first it was only slightly opake, it became completely muddy upon cooling, and remained so after being kept at rest for some days, but it was rendered nearly clear by filtration. Fourteen successive infusions were made with the same powder, when it appeared that all the matter was removed which water was capable of dissolving. The second infusion, like the first, was opake, but the 13 remaining infusions were nearly, or quite transparent. By the application of heat the opake infusions were rendered transparent, but they became opake again as they cooled. The opacity was equally produced whether they were exposed to the atmosphere, or entirely excluded from it. After being kept for some time, all the infusions became mouldy, the quantity of mould being of course greater in the earlier ones. The water was found to have dissolved ·42 of the powder; the residue was of a redder colour, and of a more spongy texture than before the experiment. The infusion seemed to retain its transparency until all the water was evaporated; the extract was hard and brittle, and of a very deep reddish brown colour. It was softened by heat, and reduced to a half melted state. From what was observed during the evaporation of the infusion, it seems that water has the power of retaining in solution almost an indefinite quantity of the extract, although when heated with the bark, it will not originally take up more than $\frac{1}{20}$th of its weight of soluble matter.

When the coccoloba bark was added to water, and no heat applied, the effect was considerably less; a transparent light brown fluid was produced, after remaining several weeks in contact; the process of moulding seemed, however, to be more rapid in this, than in the infusion made with heat.

A portion of the powder was digested with 60 times its weight of alcohol: the fluid at first assumed a dull olive colour, and gradually became more brown; the brown colour was increased by applying a gentle heat, but it still retained a considerable tinge of green. The fluid was then separated from the powder, and it gradually acquired a reddish brown colour, without any mixture of olive. The powder after being subjected to the action of the alcohol, was found to have lost exactly half its weight; the residue was not very different in its appearance from the entire powder, except that it was rather more dry and fibrous.

The warm infusion, after being filtered, and while it was still recent, was subjected to the action of different reagents, and by way of comparison, similar experiments were performed on an infusion of kino. The kino that I employed was the species commonly used in medicine, and which appeared to agree nearly with the description of the variety that is brought from Africa*. By being heated with water the greatest part of it was dissolved, but

* Thomson's Lond. Disp. 213.

after the addition of several successive portions of water, a little remained which was no longer capable of being acted on: this undissolved part was dark coloured, hard, and gritty. The infusion became opake as it cooled, but by filtration it was rendered nearly transparent. Its colour was very different from the infusion of the coccoloba, the latter being brown with a tinge of red, while the kino was red with a slight tinge of brown. The reagents employed were, 1. jelly prepared from isinglass, 2. muriate of tin, 3. oxysulphate of iron, 4. superacetate of lead, 5. tartarized antimony, 6. lime water, 7. sulphuric acid, and 8. subcarbonate of potash; the saline bodies were all in the state of saturated solution. The results were as follow:

COCCOLOBA.	KINO.
1. Copious, light brown, flocculent precipitate.	Precipitate less copious, and of a reddish colour.
2. Copious light brown precipitate.	Precipitate much less copious, and of a light buff colour.
3. Copious black precipitate.	Copious precipitate of a dull olive colour.
4. Copious precipitate, of a grayish colour.	Precipitate less copious.
5. Copious gray precipitate.	No effect produced.
6. Considerable gray precipitate.	Precipitate much less copious.
7. Copious whitish precipitate.	Precipitate much less copious.
8. Brown precipitate in moderate quantity.	No precipitate; the red colour deepened and brightened.

The tincture of the coccoloba bark was not affected by the addition of water. Jelly and the oxysulphate of iron threw down precipitates nearly similar to those from the watery infusion, but the muriate of tin had no effect. A quantity of the tincture was slowly evaporated, and a residuum was obtained which was brittle, and of a browner colour than the extract from the infusion. It appeared to be scarcely soluble in cold water, but in hot water the greatest part was dissolved; the solution became muddy as it cooled, it was rendered transparent by heat, and again grew opake when the heat was withdrawn. It was then filtered, and an infusion was obtained which was nearly transparent, and of a light brown colour. The tincture of kino being treated in the same manner, a residuum was obtained, which was of a deep red colour, and of a more friable texture than the kino itself. This residuum was, for the most part, readily soluble in hot water, although a small portion of a black substance remained undissolved. This part was soluble in alcohol; and the solution was not precipitated by water. Comparative experiments were then made upon these two infusions, by adding to each of them the same eight reagents as in the former instance; the effects were nearly similar to those mentioned above.

A portion of the residuum of the coccoloba, after the action of water upon it, was then digested in 40 times its weight of alcohol, and a light olive

tincture was produced, which gradually acquired a shade of brown. By this operation, the powder lost about $\frac{1}{8}$th of its weight. When the converse of this experiment was tried, i. e. when the residuum of the tincture was digested in water, scarcely any effect was produced, the infusion was slightly tinged by the oxysulphate of iron, and a very minute precipitate was produced by jelly.

From the above experiments we may conclude, that the extract of coccoloba is a substance of the same nature with the kino used in medicine; but that it differs from it so far as to show, that they are not derived from the same plant. They are very similar in their external appearance and physical properties; but there is an obvious difference in the colour, both of the infusion and tincture. The effect of the reagents is also different; jelly producing a brown precipitate with coccoloba, and a reddish one with kino; iron a black precipitate with coccoloba, and an olive-coloured one with kino. The effects of lime and sulphuric acid are much less with kino than with the coccoloba: there is no precipitate thrown down either by antimony or the subcarbonate of potash from kino, while the effect upon the coccoloba is very considerable. The soluble part of the coccoloba bark seems to consist principally of tan, which is in all respects similar to the tan of the gall nut; while the tan of which kino is chiefly composed, resembles that of rhatany, and the substance which is artificially pro-

cured by Mr. Hatchett's process. The tan of the coccoloba bark is accompanied, as is usually the case, with gallic acid, and it likewise exhibits those properties which have been ascribed to the presence of a specific vegetable product called extractive; but the existence of this I think has not been distinctly proved*. The substance procured by evaporating the watery infusion, is in part rendered insoluble by the process, and the same would appear to be the case with kino, as it is probable that kino is formed by evaporation, from an infusion or decoction. Its appearance indicates that it has undergone this kind of operation, and I am not acquainted with any instance in which a substance, composed principally of tan, naturally exudes from a plant.

That part of the coccoloba which is soluble in alcohol, and not soluble in water, was examined by evaporating the tincture formed from the aqueous residue of the bark. By the evaporation, a substance was left, the greatest part of which was of a greenish brown colour, and along with it a little reddish matter. Its odour was fragrant, something like that of the balsam of tolu. Water that had been digested upon it produced only a slight effect with isinglass, and the oxysulphate of iron. Neither the tincture of this substance nor of the entire extract of coccoloba were precipitated by water. The substance left, after digesting in water the re-

* See Nicholson's Journ. 24. 215—222.

sidue obtained by evaporating the tincture of the recent bark, was very similar, in its colour, odour, and consistence to that procured above: it approaches to the nature of the resinous balsam, but its solution in alcohol is not precipitated by water, although it is not itself soluble in that fluid.

The following may be regarded as an approximation to the analysis of the bark of the coccoloba:

Woody matter	50
Tan	41
Balsamic substance	9
	100

A CASE

OF

SPLENITIS,

WITH FURTHER

REMARKS ON THAT DISEASE.

By ROBERT BREE, M.D. F.R.S.

Read May 26, 1812.

HAVING laid before the Society a Case of "Painful and Tumid Spleen," which I ventured to offer as a true description of the earliest and most simple state of splenitis, I beg leave to call their attention to an example of that affection in its more advanced stage. In this part of its progress the disease may be expected to have assumed its inflammatory character, and to correspond with the account of writers on splenitis. Even in this stage I believe that the treatment may be improved. It appears that the opinions of physicians are not settled respecting this disease; since the treatment of the following case had been exceedingly various, and though it was directed by men of great intelligence and experience, the plan that was successful had not made a part of their practice; and it must

be inferred, that continued daily purgation had not been yet considered an effectual means of cure in splenitis.

The subject of this case is a young lady about 25 years of age, whose disease was first noticed in the year 1808, during a state of great uneasiness of mind. She had been sensible of debility and shortness of breath in the spring of this year, but in the summer she was free from complaint. In the month of September she began to complain of an uneasiness across the pit of the stomach, below the sternum, from the spurious ribs of one side to those of the other side, and under them. The right side was most affected, but she could not lie on either side. The pulse was not disturbed. Mr. Harcourt who attended the patient observed, that it varied from 60 to 70, and only became quicker on the advance of the disease, in fits of pain and dyspnœa. In three weeks she began to suffer pain on pressure of the left side, and the right side became more easy in proportion as this sensation increased. She now had frequent nausea and puking, but an eager desire for food in the intervals of her sickness.

The first year passed in this manner.

In the course of the next year, 1809, the disease had increased greatly, and she was attended by many of the most eminent men in our profession.

In April she was affected with cough, but there was no expectoration.

In May and June she went through the full trial of a course of mercury.

In the middle of summer there was an abatement of severity in the symptoms for a short time.

In September the disease had again increased in violence. To the symptoms already enumerated there was added a new form of dyspnœa. It now came on by spasmodic fits, and it also distressed her in the intervals as before.

In November she had some mitigation of her sufferings, but there was no suspension of her complaint. On the attacks of spasm and pain the pulse rose to 120; at other times it was seldom higher than 80 in a minute.

I was desired to visit this patient first on March 17th, 1810. About two years from the beginning of her complaint, which had now greatly increased in violence, and was attended by great depression of mind. The attacks of spasm were particularly distressing: for these she took large doses of antispasmodic and opiate medicines, which gave relief for short intervals only. Those attacks consisted of difficulty of breathing, and sense of suffocation, accompanied with faintness and confusion of the

head. When the paroxysm abated, extreme lowness succeeded; and the pain of the left side grew insupportable. With more or less of these symptoms, she had been unable to lie down for several months, and she had a heavy weight in the left side. Upon examination of the side, it was found to be enlarged and painful: the enlargement was not prominent at a point, but diffused from the margin of the false ribs to the pit of the stomach and backwards to the spine. The 8th rib, and the lower ribs appeared to be pushed forward. The least motion of the body aggravated the symptoms: the pulse had become as high as 90 between the attacks: the tongue was moist, with a whitish surface, the skin was natural, the evacuations of stool and urine were natural.

I prescribed pills composed of aloes with antimonial powder and extract of conium to be taken every six hours, with a saline draught. She was recommended to bear the attacks as far as possible without applying to opiates and antispasmodics.

On my next visit I found that she was beginning to derive benefit from this plan. The symptoms had abated gradually from the first purgative operation of the medicines, which were continued with the effect of producing numerous stools every day till the 21st of April.

April 21.—The pulse was less frequent: the

spasms were less distressing in violence and duration, and she had seldom taken the anodyne draught. The tension and soreness of the side were much less. She was directed to pursue the same medicines.

May 5, 19.—She had continued to have five or six stools daily, and to discharge much urine, but neither of these evacuations had any remarkable appearance. The swelling of the side was more diminished. The spasms were not so frequent or severe, and the dyspnœa and local pain, though yet constant, were become bearable. The pulse was 96 only during the attacks. From the general amendment, it was determined to attempt the use of a stomachic, combined with aperients. An infusion of cascarilla was directed to be taken twice in the day with soda and aloetic wine: and a saline draught was prescribed to be taken at bed time, with tartarized antimony, and camphorated tincture of opium.

June 16, 30.—After a short trial of the stomachic draughts, it had appeared that pain, general irritation, and dyspnœa were increased, and that the bowels had not been so freely operated upon as by the former medicines. She was therefore directed to resume the aloetic antimonial pills, and to take a saline draught with sulphate of potash instead of that with infusion of cascarilla.

July 14, 28.—This plan of medicine having been pursued through the month of July, all her complaints were considerably lessened by the end of the month. She left her room about this time.

August 11.—She still persisted in the evacuant plan with continued good effect. The fulness or general swelling of the side was gone, but there was a perceptible thickness of the cartilaginous extremities of the false ribs, and of their integuments, with some tenderness on the pressure of these parts, close under the false ribs, and on their edge up to the scrobiculus cordis. By the absence of the swelling internally, these external parts were more distinctly noticed as the seat of complaint. The side had been invariably more uneasy when the bowels were less purged. She had, however, been moved from four to six times every day. The pulse was still quicker than natural, but it generally was at 90 in a minute without much variation. It appeared proper to continue the plan of medicine, by taking pills of tartarized antimony and aloes every night, and a saline draught every morning.

September.—At the beginning of this month the thermometer stood at 80, and in a few days afterwards at 55 and 60, but she felt no return of disease, and could now lie very well on either side. The pulse was 84. The pills and draught had procured not less than four motions every day, which had no extraordinary appearance. She had a good

appetite, and the power of taking exercise without fatigue or dyspnœa.

As the disease had manifested itself most in the spring and autumn, I had looked to the advance of the present season with anxiety, but from the favorable state of the case at this period, I began to expect that the patient might escape a relapse. She was not, however, so far confirmed in health, or the disposition of the affected organ was not so far changed, as to realize my hope.

For several days after the 20th of September, a weight had been gradually increasing in the left side, attended with some uneasiness in lying on the right side. The head was confused and giddy, and she had a short and panting respiration, with chilliness and heat at intervals. The pulse was 92. She had lately used a fuller diet and more exercise, and the menses were expected in a few days. I hoped that this change, with the effect of active purging, might reduce the action and fulness of the vessels, and remove the attack. I prescribed larger doses of aloes united with calomel and a purging saline draught every morning. She was to take a draught of camphorated mixture with camphorated tincture of opium after the operation every night, and oftener if necessary. These means answered very well. In two days the menses appeared in great abundance, but with a natural colour, and the patient was relieved from this short attack by the end of the month.

October.—After this she remained free from complaint for several weeks.

November 14.—In the beginning of this month the disease returned with some variation of symptoms, in consequence of taking cold. It had assumed a more acute character, than I had observed since the formidable symptoms gave way in the spring; and it might have been mistaken for pleurisy by a person unacquainted with the preceding circumstances. She had a severe cough, and a pain in the side, where the complaint had been before felt. She had much head-ache, sickness of the stomach and rigors. The pulse was 110.

I ordered a saline draught to be taken every four hours, and pills of extract. coloc. compos. and gamboge to be taken directly, and repeated according to circumstances once a day. Twelve ounces of blood were taken from the arm, which had the appearance of healthy blood: there was no size, and the texture was good. The bleeding afforded very little sense of relief from pain, which extended from side to side, but was felt acutely in the left side under the false ribs. There was no swelling perceptible externally. In this state of things the menses appeared on the 21st of November, and gave very speedy and almost perfect relief. The cough was gone: she had no head-ache or sickness: the pulse was reduced to 85. She was directed to take pills as before, of aloes and tartarized antimony.

At the end of this month, she was free from complaint. The pulse was at 80. She had a perspiring skin, and evacuated many stools every day. She was directed to continue the aperient pills.

It appeared to me that the disease was stopped in September by the evacuant medicines, and the appearance of the menses, and I considered it doubtful if the relapse would have occurred in November if she had not taken cold. The attack in November had the character of acute splenitis in which the investing membranes were principally, if not wholly, the seat of the disease. This acute state was favored by the increased irritability of the organ in consequence of its long suffering, and the continued application of necessary treatment; but it seemed to be the harbinger of a confirmed disposition to sound health in the part affected.

Since the end of the year 1810, this patient has remained well, with the slight exception of feelings of uneasiness in the side on the approach of the menses, which menstruation has always removed. But she considers that the pills of aloes and tartarized antimony have been, by their effect on the bowels during the last year, a security for the confirmation of her health.

Remarks on the preceding Case.

I have considered this case as affording an example of an advanced state of splenitis. It might be called the "Second Stage" when compared with that disease which I had the honor of describing to the Society as a case of "painful and turgid spleen" in its most simple state. In the earliest condition of this disorder the organ is swelled from the passive state of its vessels, which receive a greater proportion of blood than they can return. No fever accompanied this stage, nor was it the effect of fever, but an idiopathic affection, leading to inflammation by tension and irritation of the membranes that invest the spleen. The means of cure were experienced to be active and daily evacuations, so persisted in as to become the probable occasion of disease, if they had not been essential to the removal of it. A considerable length of time is necessary to the cure of this affection, because it is dependent on the weakness and passive condition of the vessels of the spleen, and its duration must be in proportion as the return of their energy and contractile power is slow. If the second stage of splenitis be made free from danger, it must require still longer time before it terminates in a cure than the first stage, as the disposition to a renewal of vascular swelling will be more confirmed by time, and show itself periodically.

The more advanced stage of this disease has been often described, and too often furnished the anatomist with observations after death.

In the second stage the pulse becomes quicker, and it is long, in convalescence, before it is reduced to its natural standard. The increased pulse is produced by painful irritation at first, and next by the actual tension of the membranes, proceeding to inflammation and adhesion of adjoining parts. The quickness of the pulse will assist in distinguishing the degree of progress of this disease, for it will be found, by reference to histories, that in a great proportion of cases, there was no warning of the growing mischief in its earliest stage; and that painful affection of the left side existed in many other cases, long before fever was induced, though these ended fatally.

In the first stage the patient can lie upon the left side, but not on the right side. In the second stage it is impossible to lie on the side affected. The spasmodic action of the diaphragm is more likely to come on in the second stage, and may be much aggravated by stimulant treatment. There is no emaciation in the first stage of a morbid kind, nor any considerable emaciation in the second stage, notwithstanding the large and continued evacuations. But in the third and last stage of splenitis, emaciation is always an attending symptom, combined with hectic or slow fever, particularly in middle-aged and elderly people. In this third stage diarrhœa supervenes, as well as dysentery, and discharges of grumous and dark blood take place, by vomiting and by stools: these

charges give temporary relief in many cases, and occur long before the final event.

Besides the two cases which I have submitted to the Society, it is right to observe, that I have seen the result of many others during the last three years, three of which occurred in young men and the majority in women. All these have concurred to shew me in a satisfactory manner that drastic purging, long continued, is the proper mode of treatment. By this practice a young woman has been relieved of a swelling of the spleen and epileptic fits at the same time. The fits began with the first symptoms of disease in the left side, and have disappeared for the last year, during which time she was gradually recovered from the swelling and pain.

Compositions of aloes and antimony were preferred in the cases that have been related, and generally in others, but not exclusively adopted: large doses of neutral salts have however appeared exceptionable when exhibited daily, as they have occasioned flatulence and depression. But aloes, extract of colocynth, and scammony with jalap, have acted without this inconvenience, and calomel has been combined with these, at intervals, producing more effectual discharges from the bowels: tartarized antimony in such minute doses, as not to puke, has always appeared to increase the beneficial effect of those combinations.

This idiopathic disease affects females more than males, and they have been always relieved in a signal manner by the flow of the menses: this fact may give rise to an idea somewhat speculative, but not without support, that a particular correspondence or sympathy of action, may exist between the spleen and the uterus. Each of these vascular organs have a structure which yields to extension and contraction with less disturbance of the whole habit than happens from equal changes in other organs, and wherever splenitis is relieved by nature or art, the inferior vessels of the body receive more than their usual quantity of blood, which passes from the uterus or the intestines: a fact agreeing with the 48th Aphor. of Hippocrates, lib. 6. "Τοισι σπληνωδεσι &c. Splenicis dysenteria superveniens salutaris."

The progressive effects, and the complications of this complaint may appear after death; but it is obvious that no certain conclusions can be drawn from dissections respecting the two first stages of it, and particularly that the condition of the organ in the earliest and most simple state of its suffering cannot be proved by anatomical examination.

Splenitis, complicated with extensive visceral obstructions, has been very frequently the effect of remittent and endemic fever; but a tumid spleen occasioned by this cause, cannot afford any reasoning respecting the idiopathic disease, which in its

progress will be distinguished by a hectic fever, of a very different character. The difference between such complicated affections and this simple disease will appear sufficiently manifest to those who consult the histories of Lieutaud and Morgagni, but particularly the recent descriptions of Dr. Davis in his treatise on the Walcheren fever.

Aretæus says, that the disease is fatal to elderly people, but that it is not so dangerous to youth. Dr. Heberden's later observations confirm the assertion of Aretæus respecting the tendency of the disease in people advanced to middle age, in whom it is fatal in six months.

It is very important at every period of life to distinguish the disease in its early stages; we are not to expect the presence of pyrexia, and external swelling to make the case obvious. Pain of the left side, and inability to lie on the right side, are often the only signs, and if these two symptoms exist without fever, there cannot be much doubt respecting the seat of the disease, for a painful disease of any other organ of the left side would be accompanied with fever, and other symptoms of constitutional sympathy and irritation.

The spleen has been described very generally as a passive organ, and not readily sensible to inflammatory irritation. (See Dr. Pemberton.) This character can occasion little wonder, if the structure

and œconomy of the spleen be well considered. (See Malpighi and Hakely &c.) The absence of pyrexia in the early part of splenitis does not appear so remarkable as it does in cases of the strangulated intestine, in which Dr. Baillie says, "The pulse is sometimes not increased in frequency beyond the standard of health, and yet the inflammation of the bowels has been discovered afterwards by the operation to be very great."

It is impossible to say how far a continued application of purgatives might have answered in advanced cases, or in the first stages of cases, which ended fatally, as this practice has not been followed in any instance within my knowledge. In a case detailed in the Edinburgh Journal, it was found that purging was the best means of relieving the patient, but it was only practised at intervals, and it was accompanied by the use of medicines which must have acted upon an opposite principle to that of evacuation, possibly counteracting its influence. Similar cases are to be found in Morgagni and Lieutaud, and it must appear from the result, that it was a third stage of splenitis when the ingenious writer first saw the case, the distinguishing character of the early part of the disease having been lost. The patience and insensibility of the organ is so remarkable that changes that must have been of long standing, have been only discovered at the death of the patient.

A person met with sudden death from accident, and, as it is related in the "Acta Parisiensia," his spleen was found *stoney* in its internal structure without any other mark of disease.

Enlargements are recorded by writers to have existed without notice of the change, and fever, or severe pain only came on to alarm the patients after a long period, though these diseases terminated in death.

It is therefore true in the distinguishing of splenitis, that the disease may have made great progress without observation. Pain may have been felt without swelling, and swelling without pain, and both may have existed without the pulse having been disturbed for a long period in the first stage of splenitis; but on dissection, the spleen, after such previous circumstances, has been found morbid, and the only diseased organ.

ACCOUNT

OF THE

MUSCLES OF THE URETERS;

AND

THEIR EFFECTS

IN THE IRRITABLE STATES OF THE BLADDER,

By CHARLES BELL, Esq. F. R. S. Ed.
TEACHER OF ANATOMY IN GREAT WINDMILL STREET.

COMMUNICATED BY

DR. CURRY.

Read June 23, 1812.

I am about to describe a set of muscles which seem not to have been observed by former anatomists. They are attached to the orifices of the ureters, and are seated in the bladder. In health they are the instruments of a very peculiar organic action, and in disease the cause of most distressing complaints.

Before I proceed to describe the anatomy of this part of the bladder, the subject seems to require, that I should give a short historical review of the opinions respecting it.

Of the parts hitherto described as seated at the neck of the bladder—La trigone de la vessie—La Luette—Uvula Vesicæ—Corpora carnosa Morgagni.—Third lobe of the prostate, &c. &c.

In the plates of *De Graaff* there are represented certain folds extending forward from the orifices of the ureters, where they terminate in the cavity of the bladder; and at the lower part of the orifice of the bladder, there is a tubercle faintly indicated. The same appearance is represented by *Bidloo*. In *Santorini* also the natural appearance of these parts is accurately delineated. *Morgagni* expresses himself to this purpose: "at the points where the ureters terminate in the bladder, there arises from each of them a thick round compact fleshy body, which takes a direction towards the orifice of the bladder. These two bodies having proceeded a little way, are united, and proceed forward, terminating in the *Caput Gallinaceum* *."

Santorini † gives the same description of these parts as Morgagni has delivered.

Lieutaud describes these bodies under the term *La trigone de la vessie*. The learned Portal is incorrect in saying that Lieutaud was the first anatomist who has given their description.

* See Morgagni Adversaria I. n. 9. Adversaria III. Animadver. XLII.

† In the Observationes anatomicæ, cap. x. sec. xxi.

Portal has thus described the *Trigone:* at the lower part, the internal tunic of the bladder adheres to a triangular body of a cartilaginous hardness, and this body is always prominent in the cavity of the bladder, especially in old men. He proceeds to say, that, at the extremity of the triangle, backwards, the orifices of the ureters open; and at their anterior extremity, there is an eminence slightly protuberant, to which Lieutaud has given the name of *Luette.*

This account leads me again to refer to the plate of the excellent anatomist *Dominico Santorini.* In his second table the Luette and Trigone are accurately represented.

He has the following explanation on the letter I. " Vesicæ urinæ osculum cui prominulum corpus " præfigitur quod in affectis vesicæ sic prominet ali- " quando ut urinæ iter prorsus intercludat." This refers to the disease with which Mr. Hunter and Mr. Home have made us familiar.

The expression of Santorini recalls us again to the remark of Portal: he says. " I have found in " old men who have suffered retention of urine, " the *Trigone de la Vessie* so enlarged, especially " its tubercle, in the form of an Uvula (Luette) " that the orifice of the bladder was shut by it*.'

* Portal, Cours d'Anatomie Med. T. v. p. 409.

Sabatier follows his countryman in his description of this part of the bladder, but adds; " The *Tri-* " *gone* and *Luette* are the most sensible parts of the " bladder; which is the cause why a stone lodging " here produces extreme irritation, while if it " lodges in any other part of the cavity of the blad- " der, it causes little inconvenience:" he adds, " The uvula *(Luette,)* which terminates the ante- " rior angle is very subject to swell, and then it " rises in the form of a round tumor which fills the " neck of the bladder, and opposes itself to the flow " of urine *."

Desault, speaking of the tumors which grow in the bladder, has this expression. " Le sommet de " ce viscère n'en est pas plus exempt que son bas- " fond; mais ce sont particulièrement ceux qui crois- " sent près de son col, et que quelques auteurs ont " pris pour un gonflement de la luette vesicale, qui " occasionnent la retention d'urine."

This sentence, which betrays the imperfect knowledge which Desault had of the disease, is followed by other unequivocal marks of unconfirmed principles and practice: and the whole chapter stands in remarkable contrast with the publication of Mr. Home, in this country.

* See also Lieutaud, Hist. Anatomic. medica. tumores vesicæ adnati.

In Haller's *Element. Physiolog**. we have a description following that of Morgagni under the title *Colliculi ab Uteribus in Urethram producti.*

These authorities discussed, we come now to the more modern observation of Mr. Hunter.

Mr. Hunter† has described a small portion of the prostate gland which lies behind the very beginning of the urethra; and this he describes as subject to swell out like a point into the cavity of the bladder, where it acts like a valve on the mouth of the urethra. This can be seen even when the swelling is not considerable, by looking upon the mouth of the urethra from the cavity of the bladder.

It is impossible to mistake this; the swelling he describes is the *Uvula Vesicæ* or *Luette* of Lieutaud. The observations of Mr. Hunter, then, go to inform us, that this tumor is of that part of the prostate gland which is below the urethra, and betwixt the lateral portions of the gland.

This discovery carries us back to the great anatomists in whose works we find the elements of all our present knowledge. Morgagni has very fully described the part of the prostate gland which Mr. Hunter mentions, and which he discovered to be the seat of this dangerous malady. Morgagni thus describes it:

* T. v. p. 328. † P. 170.

" Quod si vera ulla propago prostatæ addenda " est, ea certe est subrotunda et renitens quasi " glandula, quam cum sæpe diligentissimus inci- " sor noster in publicis dissectionibus animadvert- " isset inter vesicam, et seminales capsulas qua " sese mutuo hæ jam contingunt, prominentem " et nonnunquam ad angulum conveniendo, effici- " unt, prostantem nos accurato instituto examine " nihil aliud esse comperimus quam corporis ip- " sius prostatæ particulam.*"

In addition to the description of Morgagni we have the authority of Sabatier. "Sometimes," says he, "only that part of the prostate is diseased to " which they have given the name of *Luette Vesi-* " *cale.* I have seen several occasions (he continues) in which the uvula forms a tumor with a " narrow peduncle: this moving with a stream of " urine, closed the opening of the bladder†".

Anatomy of the Neck of the Bladder.

I have now to lay before the Society the anatomy of the neck of the bladder in man.

On dissecting up the inner coat of the bladder, there are seen two strong fleshy columns, which

* Morgagni, adversaria anat. IV. animad. XV.

† Med. Operat. T. II. p. 72.

descend from the orifices of the ureters towards the orifice of the bladder: they unite and run towards the prostate gland. On the surface, towards the cavity of the bladder, they are denser by the union of the inner coat of the bladder, but they are fibrous, and this fibrous structure is made manifest by dissection from below. They are larger and firmer, but of the same colour and structure with the fleshy columns of the *Detrusor urinæ*. The variety which we find in their length according with the degree of contraction of the bladder, proves their muscularity. Whatever excites the action of the bladder increases the size of these muscles in a remarkable degree, and they always acquire a great increase of power and size when the muscular coat of the bladder, becomes more distinct and powerful. In some of my specimens of diseased bladder, I find the cause of this to be stone in the bladder; in others, an ulcer; in many, stricture; but always irritation and the necessity of continual action of the bladder are attended with an enlargement of the muscles of the ureters.

When contracted, the course of these columns is distinguishable all the way from the mouths of the ureters to the beginning of the urethra; and there, at their union, they heave up the inner coat of the bladder producing the appearance of a tubercle at the lower part of the orifice of the bladder*.

* It is this appearance presented by the muscles in a state of contraction, which has led so many of our best authorities to confound it with the disease of the third lobe of the prostate gland.

It is still the form of the inner coat which makes these fleshy columns appear to terminate forward in the caput gallinaceum; which they do not; they only take a firmer insertion. Where these columns unite they are most fleshy, and their fibres are more intricate; then, directing their course towards the lower and backmost part of the prostate, they degenerate into tendon, and are inserted into the portion called the third lobe of the prostate.

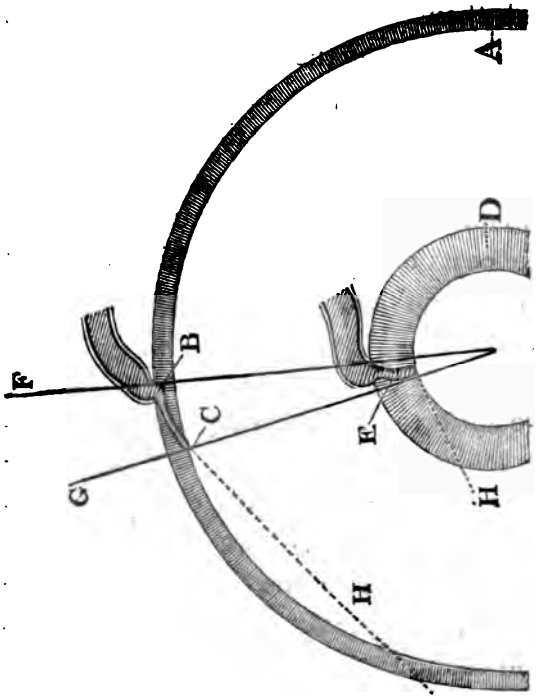
Although I have described the course of these muscles as proceeding from the back part forward, because it better corresponds with the first view we have of them, yet, I believe, it is more correct to consider their connection with the prostate gland as the fixed point, and their connection with the extremities of the ureters as their insertion.

USE.—The use of these muscles is, to assist in the contraction of the bladder, and at the same time to close and support the mouths of the ureters. The surface of the bladder where it covers their union on the inside, is endowed with an exquisite sensibility, which is a provision of nature, for their ready and instantaneous action on the stimulus to pass the urine. It is here that is seated that sensibility which produces the natural call to urine, and here also is the seat of diseased irritations.

It will be observed that the orifices of the ureters are not closed by the contraction of the muscular

fibres around them. They are defended against the return of the urine by the obliquity of their passage through the coats of the bladder. It is well known that the extremity of the ureter enters through the coats of the bladder obliquely, and that in consequence of this, there is a valvular action in the coats of the bladder which prevents the regurgitation of the urine into the ducts of the kidney.

But if we look to the adjoining diagram, and consider the subject, we shall find, that in proportion as the bladder contracts, this obliquity must be diminished; and further, if we reflect that the coat which contracts is on the outside of the oblique passage of the ureter, we shall conclude that without some counteracting power on the inside of the bladder to draw down the orifice of the ureter, the obliquity of the passage would be lost. These muscles, which I have now described, guard the orifices of the ureters by preserving the obliquity of the passage, and by pulling down the extremities of the ureters according to the degree of the contraction of the bladder generally.



Explanation of the Diagram.

Let A. represent the circle of the dilated bladder.

B. The *Ureter* or duct of the kidney of entering the coats of the bladder.

C. The extremity of the duct opening on the inside of the bladder.

B. C. mark the oblique course of the *Ureter* through the coats of the bladder.

Let D. represent the contracted bladder, thickened at the same time by its contractions.

E. The *Ureter* passing through the coats.

The lines F. G. drawn from the centre of the circle will intersect corresponding portions of both circles, and demonstrate how the oblique passage of the *Ureter* through the coats of the dilated bladder becomes more direct in the contracted bladder.

The muscles described act in the direction of the line H, and its

The membrane which covers these muscles, is the seat of that sense which calls the muscular coat of the bladder into action. Of this we may be sensible in passing the bougie, and still more in passing the urethra sound. As the instrument is passed down the urethra, there is a sickening sensation; as it passes the caput gallinaceum, the nature of the pain is changed; and, lastly, in passing it over the surface of the triangular elevation, produced by these muscles, there is experienced the familiar sensation of the call to pass urine. If it were doubted that here, in a particular manner, is seated that sensibility, which calls the bladder into action, the effect of a stone falling upon the part is sufficient proof. When a patient has a stone in the bladder, there is pain and excitement while it rests in this place, and relief when it lodges elsewhere. The reason why this part is possessed of such sensibility, I apprehend to be, that the muscles of the ureters may, as it were, be the first alarmed, in order to guard the mouth of the ureters, and preserve their obliquity during the action of the bladder.

its operation is to draw down the orifice of the *Ureter* C. in proportion as the bladder contracts, by which means the obliquity of the passage is preserved.

Action in Disease.

When the sensibility in the seat of these muscles is increased by disease, and the increased sensibility is accompanied with a continual action of the muscles, the prostate gland must suffer unusual excitement. The natural prominence formed by the muscles being directly over the third lobe of the prostate, and their chief attachment being also to this third lobe, we may perceive how it happens that this part is sometimes enlarged without the body of the prostate gland partaking much in the disease. When there is an unusual *nisus* of the bladder, these muscles are the seat of it; and as their united extremities are attached to the lower and middle portion of the prostate gland, they must, I think, promote the growth of this portion in a direction towards the cavity of the bladder. This will produce the true *uvula vesicæ*, the pendulous tumor in the neck of the bladder resembling the uvula of the palate. This tumor hangs into the cavity of the bladder, and falls like a valve upon the orifice of the bladder, proving a most troublesome and dangerous obstruction to the urine.

But whilst I state this as an opinion, drawn from the consideration of the parts in their natural state, I must also submit the naked facts. I have, in my collection, two specimens of the disease of the third lobe of the prostate, where these muscles are remarkably strong. I have, on the

other hand, some specimens of diseased bladder, where the muscles of the ureters are enlarged; and only in one of these is there a beginning enlargement of the middle lobe of the prostate gland.

I have many specimens of bone distorted by the action of the muscles; and many, where, at the attachments of the tendons, the bones are drawn out into spines and tubercles. We may say, such is the effect of the muscles; but though the growth of such spines or tubercles be the effect of the action of the muscles directly, yet these spines will not be formed, unless when the bones are at the same time suffering from disease. So in considering the action of the muscles of the ureters, as influencing the growth of the prostate in a particular direction, I do not imagine that the muscles will do this merely by their mechanical effect. There must be also a disposition to disease in the prostate; and if disease be not present, the irritation of the bladder will continue even till the death of the patient, without affecting the growth of the gland.

On the extremities of the ureters, in the diseased action of the bladder, the contraction of these muscles is converted from a salutary influence, to one which aggravates disease. They still close the mouths of the ureters during the action of the bladder; and the action continuing, they cause an accumulation in the ureters and pel-

vis of the kidney, and influence the kidney itself: thus increasing the extent of the local disorder, and consequently its influence on the constitution.

Such consequences as arise from the irritation and action of these muscles, are to be relieved by removing the cause; by assuaging the sensibility of the surface of the tubercle, and by drawing off the urine. When we know that this spot on the lower part of the orifice of the bladder, is the seat of that irritability which is so distressing, we see that it is practicable to effect it by the use of the bougie. By the introduction of the catheter, the urine is let off without hindrance from the valve: the distressing excitement of the muscles is not perpetuated, and the prostate subsides from its irritation.

This practice, though a direct deduction from the examination I have made, is no more than the advice given to us by the best authors.

Thus we see that a small tubercle was painted by De Graff, and described by others, but the nature and origin of the tubercle was misconceived. Anatomists were calling it *uvula vesicæ*, as if that natural prominence was the same with the *tumor* of this part of the bladder. We find that the disease called *uvula vesicæ*, is no other than that occasioned by the enlargement of the third lobe of the prostate gland. To Mr. Hunter and Mr.

Home, we are indebted for a full knowledge of the nature of this disease. It has been objected to Mr. Home, that he is in vain making that important, which the great anatomists of all ages have failed to discover, or have neglected to notice. I have shewn that the third lobe of the prostate was known to Morgagni, and that it was a subject of discussion in his day. We have sufficient evidence (even on this very subject) of the difference of a fact being noticed in the elaborate works of Morgagni, and of its being familiarly and practically known to surgeons. The third lobe of the prostate was quite forgotten, the consequence was that we were ignorant of the nature of the most fatal disease of the bladder. It would be disingenuously reserving the circumstance, which drew me to attend to this subject, were I to omit the mention of the late work of Mr. Home, or rather his original paper on the anatomy of the prostate gland. I acknowledge both the merit and the necessity of what he has written: for the observation of Morgagni, and the hint of Sabatier, were forgotten both here and in France, until the subject was distinctly and practically brought forward by Mr. Home. Even the latest French author, Richerand*, still speaks of the enlargement of the anterior angle of the "*trigone vesicale*," and the growth of *fungosities* near the neck of the bladder, obstructing the course of the urine.

* Nosographie Chirurgicale, ii^e^. ed. p. 458.

It still remained to be explained, why the small part of the prostate gland, the third lobe of Mr. Home, should be so frequently enlarged, without the affection of the body generally being apparent, or why this part should enlarge more rapidly than the rest of the gland. It was in the prosecution of this inquiry, that I discovered the muscles of the ureters, and after ascertaining their nature, I saw through the obscurities of authors, in treating of the diseases of this part of the bladder; so that in speaking of the enlargement of the anterior extremity of a natural tubercle, they were deceived, and that, in treating of the *uvula*, they were describing the diseased prostate gland.

EXPLANATION OF THE PLATES.

PLATE IV.

A portion of the inside of the bladder, with the prostate gland appended to it, by the attachment of the common tendon of the muscles of the ureters.

A. The inside of a portion of the bladder.

B.B. The mouths of the ureters opening into the bladder.

C.C. The *muscles of the ureters;* to shew which more distinctly, the inner coat of the bladder is dissected up, and a bristle is put under them.

D. The union of the two muscles;—a bristle is put under this part.

E. The tendon of the united muscles of the ureters inserted into the prostate gland.

F. The middle lobe of the prostate gland, into which the muscles of the ureters are inserted: all other connection betwixt the bladder and the prostate gland is cut through, that the gland may hang and stretch out the muscle.

G. The caput gallinaceum, and part of the urethra.

PLATE V.

The lower part and neck of the bladder, the vesiculæ seminales, the prostate gland, the membranous part of the urethra, and (from the lower side) the muscles of the ureters.

A. The spongy body of the urethra cut across.

B. That part of the spongy body called the bulb.

C. Cowper's gland.

D. Membranous part of the urethra.

E. The prostate gland.

F. The extremity of the vasa deferentia, passing through the prostate gland, to open on the caput gallinaceum.

G.G. The vesiculæ seminales.

H.H. The vasa deferentia, where they are near the ureters.

I.I. The ureters, or the ducts of the kidney leading to the bladder.

K. The prostate gland, where it is betwixt the urethra and seminal vessels; this is the third lobe.

L. The outer coat of muscular fibres cut across.

M. The outer coat of muscular fibres of the bladder; a portion of these fibres is cut across, to shew the muscles of the ureters.

N.N. The muscles of the ureters seen on the lower surface; their fibrous structure is more evident on this lower surface.

O. The union of the muscles where their fibres are more intricate.

P. The thin inner coat of the bladder, with some few transverse fibres.

PLATE VI.

This plate represents the diseased prostate gland and part of the bladder. The third lobe of the prostate is enlarged, and the connection of the muscles of the ureters with it is very distinct.

A.A. The lateral portions of the PROSTATE GLAND.

B. The *caput gallinaceum.*

C. The *third lobe* of *the prostate gland*, considerably enlarged and prominent.

D. The union of the *muscles of the ureters*. Their attachment to the third lobe, C, is very distinct.

E. A piece of bougie passed under the muscles.

F.F. The orifices of the ureters.

This being a fair specimen of disease of the third lobe of the prostate gland in its first stage, I thought I could not have a better opportunity of demonstrating the connection of the muscles of the ureters with the prostate gland, and I therefore dissected up the inner coat of the bladder. This plate at the same time presents a fair specimen of the muscles, when enlarged by continued excitement. I possess several preparations where they appear as strong and distinct as here without dissection.

HISTORY OF

A CASE

IN WHICH

A CALCULUS WAS VOIDED

FROM

A TUMOR IN THE GROIN.

By T. COPELAND, Esq.

ASSISTANT SURGEON TO THE WESTMINSTER GENERAL DISPENSARY.

Read Sept. 8, 1812.

THE relation of extraordinary occurrences and insulated cases, does not usually contribute much to the systematic advancement of chirurgical knowledge. But as the disease which I am about to describe, appears to suggest some important reflections, and lead to a more careful prognosis in similar complaints, it may not be unworthy the attention of the Society.

Monsieur Petit has related, in the Academy of Surgery, many cases of external tumor formed by the gall bladder in the right hypochondriac region. Some of these tumors being mistaken for simple abscess, were opened and proved fatal; in

some of the cases the gall bladder adhered to, and broke into, the colon; in some abscess was formed externally, which broke, discharged gall stones, and remained fistulous; and in other cases they entirely healed. The symptoms which distinguish this species of tumor from others, are detailed with the singular sagacity which marks the writings of that great surgeon.

Although in the following cases, the gall bladder was not immediately concerned, they seem so evidently allied to the subject of Mons. Petit's Essay, that it is impossible to avoid referring to it.

A lady, about fifty years of age, was affected with great pain, which was described to be in the stomach, constant sickness, and a costive state of the bowels, for which she took such medicines as were prescribed by her physician. These symptoms had continued for many weeks, when a tumor, evidently containing a fluid, presented itself just above the right groin, which I was desired to see. The preceding history, the want of surrounding inflammation which should have characterized a simple abscess, and the recollection of Petit's* paper, induced me to decline opening the tumor.

* It will be remarked by those who are not aware to how great a size the gall bladder will arrive, that the situation of the tumor distinguished it from such an affection. I may therefore observe, that there is in the collection of St. Thomas's Hospital, a human gall bladder, which, I suppose, would contain at least three pints of fluid.

I could not suppose it was a lumbar abscess, the only other disease for which it could easily be taken; and even the propriety of opening such tumors is, I think, by no means fully established. A poultice was therefore applied, and in about ten days it broke, and discharged a considerable quantity of discoloured matter. The wound became exceedingly irritable, the granulations were loose, indistinct, and dark-coloured, and the patient's health much impaired. She was advised to go into the country, and some time after her departure, I was informed that the complaint had assumed a different character, that the discharge had become fœtid, and that the caraway-seeds in some cakes she had eaten, were occasionally seen with the discharge on the lint covering her sore. Soon after this, she returned to town, and another abscess formed and communicated with the preceding one, and the discharge became manifestly fæcal. Meanwhile the functions of the bowels were not materially impeded. She had an evacuation daily, with or without aperient medicine, but her health suffered much from the irritable state of the sores, which was considerably increased by attempts at locomotion.

At this period of her complaint, when I was examining the wound deeply with the probe, I felt it sensibly strike against a hard body, which, with some difficulty, I could move so as to ascertain tolerably its situation and size. I then enlarged the

fistulous sore with the bistoury, and, with a pair of long forceps, succeeded in extracting a calculus of an oval form, the long diameter of which was more than half an inch. I did not think it necessary, like Mons. Saurau*, to remove the callous circumference of the sore, but the operation was painful and somewhat tedious, from the difficulty of seizing the stone with the forceps. No fever of consequence, or any other untoward circumstance followed, nor was the fæcal discharge more copious from the enlarged wound, a circumstance which surprised me. About this time her health began to amend, and the discharge to abate. It was found necessary a short time afterwards, to divide the interval under which the two sores communicated, and which by its friction kept the parts in a state of irritation. The wound then gradually contracted, assumed a more healthy appearance, and had nearly closed when she went into the country, and her health was so much improved, that I have no doubt, in a very short time her cure was perfectly accomplished.

A woman, about twenty-five years of age, had for some time complained of pain and sickness in the stomach, occasionally more or less violent, and the functions of her bowels were so imperfectly performed, that for many months she had been constantly compelled to have recourse to purga-

* Académie de Chirurg. Vol. I. p. 185.

tive medicines: at last a tumor presented itself which I was requested to see, just above the right groin. It was evidently situated under the integuments and muscles of the abdomen, slightly moveable, and could almost be comprehended between the fingers and thumb. There was an inflamed line of above an inch in diameter, extending obliquely upwards, and towards the linea alba. It was painful to the touch, and she had some fever.

The first impression on my mind, was that it might probably be an affection of the ovarium. But her age, the preceding history, and the very strong resemblance of the symptoms to the case I have described, induced me to suspect that, unless it were prevented, it would have the same progress and issue; for it was clearly, I think, connected with the functions of the intestinal canal. I therefore directed her to apply leeches to the tumor, which was done four or five times, and to be smartly purged every morning. With this treatment, together with rest, and a very low regimen, the tumor soon became less painful and smaller, and in the course of ten days was so far reduced as to cease to be inconvenient to her; there was, however, still a considerable induration, which could be discovered by hard pressure on the abdomen.

It would have been favourable to the analogy, which I yet presume will be acknowledged, of these

two cases, if a calculus, wherever it had been lodged, had been discovered in the fæces which passed while the tumor was diminishing. But no such concretion was seen, nor perhaps very accurately sought for, although I had said that very probably it would be found in the evacuations.

From the situation and all the circumstances of the first case, I think there can be no doubt that the calculus was the cause of the abscess, and that it was lodged in the cœcum, where that portion of the canal is attached to the right side, and not, like Petit's cases, in the gall bladder. The fæcal evacuations sufficiently distinguish the two diseases, and the fœtor of the discharge shews that it was not in the small intestines.

Had the abscess been opened artificially, I have great doubt whether it would have terminated so favourably, for the difference between a spontaneous wound, and one made by a cutting instrument, is often greater than *a priori* might have been suspected, more particularly where a serous membrane is involved in the operation; and the flow of fæces after the lancet, unless it had been distinctly foreseen and foretold, would, at all events, have been an embarrassing and unfavourable circumstance to the surgeon.

The fortunate issue of the second case, happily prevented my having ocular evidence of its precise

nature, but I cannot help supposing, from the close resemblance of the symptoms, that it was of a similar description.

Golden Square,
August 7th, 1812.

P.S. *Dr. Marcet, having been requested to examine the calculus, found it to be of the fusible kind, and indestructible by fire, and having the general properties of calculi, which consist of the mixed phosphat of lime and ammoniaco-magnesian phosphat.*

GENERAL VIEWS

OF THE

COMPOSITION

OF

ANIMAL FLUIDS.

By J. BERZELIUS, M.D.

PROFESSOR OF CHEMISTRY IN THE COLLEGE OF MEDICINE AT STOCKHOLM.

COMMUNICATED BY

DR. MARCET.

Read September 8, 1812.

HAVING related to my friend Dr. Marcet, some observations that I have made on the subject of Animal Chemistry; and being invited by him to communicate them to the Medical and Chirurgical Society, I shall, in compliance with his wish, venture to submit to the Society, some of the principal results that I have obtained at different periods, prior to my visit to this country, respecting the Fluids of Animals. Most of these observations have been published in a more unconnected state in different works in the Swedish language*; but

* In my "Förelasningar i Djurkemien, 2 Vol. Stockh. 1808. And also in Afhandlingar i Fysik, Kemi och Mineralogie, 3 Vol. Stockh. 1810."

as they have not been translated into any other language, and as they have appeared to those who have seen them, to contain some new views, I am induced to offer them to the Society, in the hope that they will be received with indulgence.

I. *Of the blood.*

In most of the analytical researches on blood, that of the bullock has been made the subject of experiment. I shall therefore begin with the analysis of the blood of that animal, and afterwards notice the essential points in which I have found it to differ from the human.

A. *Bullock's blood.*

Blood may be regarded as a liquid holding a colouring matter suspended in it, but not dissolved. The first step in the process of accurate analysis should therefore be to separate the suspended matter by filtration. But this method succeeds only to a certain degree, and requires a time so considerable, that the blood undergoes spontaneous changes of composition before the separation can be completed: for notwithstanding all possible care, the colouring matter will either pass through with the fluid portion, or by adhering in masses, prevent all further percolation. Another mode is that of allowing it to subside by rest: but this also goes on with extreme slowness: the clear supernatant liquor loses its red colour but very gradually; and the colourless portion is not capable of being collected alone.

The usual way of obtaining them separate is to take advantage of the coagulation of the blood, during which the fibrin enveloping the colouring substance, presses out the serum. This method is indeed but very imperfect, as a large portion of the serum still remains attached to the red globules in the coagulum; but it is the only one that we can employ.

I shall first consider the crassamentum, and its two constituent parts, fibrin and colouring matter.

The Chemical Properties of Fibrin.

Fibrin is insoluble in cold water. In boiling water it curls up, and after the ebullition has continued some hours, the water acquires a milky hue, but no gaseous product appears. By this operation fibrin undergoes a species of decomposition; the water in which it is boiled affords, by the addition of tannin, a precipitate of white and distinct flocculi, which do not cohere together by the heat, as those produced by gelatin. The evaporated liquid does not gelatinize to whatever degree it may be concentrated, and leaves a white, dry, hard and friable residue, which is soluble in cold water, and has an agreeable taste similar to fresh broth, and totally unlike the salt and acrid flavour of the extract from muscles. Fibrin, by long boiling in water, loses its property of softening and dissolving in acetic acid.

2. In *alcohol* of specific gravity 0.81 fibrin undergoes a species of decomposition, and forms an adipocirous matter, soluble in alcohol, and precipitated by the addition of water; having often a very strong and unpleasant odour. The alcoholic solution leaves on evaporation a fat residue, which did not pre-exist in the fibrin, and which, as we shall find, is likewise formed by the action of alcohol in the colouring matter and on the albumen. Fibrin heated in alcohol, retains its property of softening and dissolving in acetic acid.

3. By the action of *ether*, fibrin is converted into an adipocirous mass similar to the preceding, but in much greater abundance, and having a much stronger and more disagreeable odour. We are on this account precluded from employing generally either alcohol or ether in analytical experiments on animal substances.

4. In concentrated *acetic acid*, fibrin becomes immediately soft, transparent, and with the assistance of heat is converted into a tremulous jelly. By adding water and warming it, this jelly is completely dissolved, with the evolution of a small quantity of azotic gas. The solution is colourless, of a mawkish and slightly acid taste. During its evaporation a transparent membrane appears on the surface, and after a certain degree of approximation, the gelatinous substance is again re-produced; but this gelatine has no resemblance to that formed by paste.

When completely desiccated it is a transparent mass which reddens turnsol paper, but which without a fresh addition of acetic acid is insoluble both in cold and boiling water. The solution of fibrin in acetic acid added to prussiate of potass or of ammonia, gives a white precipitate, without any separation of prussic acid. This solution will also produce a precipitate by alkalies, but it is redissolved by a small excess of the latter. Sulphuric, nitric, and muriatic acids afford likewise a precipitate with this solution: and the precipitate is composed, as we shall presently see, of fibrin and the acid employed. If the precipitate be laid on a filter and washed, a certain quantity of this acid is carried off by the water, and the remaining substance is soluble in pure water. The solution contains a neutral combination of fibrine with the mineral acid employed, which is mucous, somewhat opaline, and of an acidalous taste. An addition of acid will again precipitate it, and it thus often happens that an animal substance that has been treated with a mineral acid and washed on the filter, gives at length a clear liquor which becomes turbid on falling into the acid liquor that had first gone through. This phenomenon always indicates the presence of the abovementioned combination of a mineral acid with fibrin, or with albumen, which appears to possess the same chemical properties as fibrin*.

* It may be observed that the precipitate produced by nitric acid, assumes a yellow colour, but has in other respects the same properties as the two others.

4. In weak *muriatic acid*, fibrin shrinks and gives out a small quantity of azotic gas; but scarcely any portion is dissolved even by boiling: neither does the acid liquor afford any precipitate with ammonia, or with prussiate of potass. Evaporated to dryness a brownish residue is obtained, from which potash disengages a little ammonia. Concentrated muriatic acid decomposes fibrin by coction, and produces a red or violet-coloured solution.

The fibrin that has been digested with weak muriatic acid is hard and shrivelled. By washing repeatedly with water, it is at length converted into a gelatinous mass, which is perfectly soluble in tepid water. The solution powerfully reddens litmus paper, and yields a precipitate with acids as well as with alkalies. Fibrin has therefore the property of combining with muriatic acid in two proportions. The one gives a neutral combination soluble in water; the other, a combination with excess of acid which is insoluble, but which is reduced to the state of the soluble compound by the action of pure water.

5. Concentrated *sulphuric acid* decomposes and carbonises fibrin. The same acid diluted with six times its weight of water, and digested with fibrin, acquires a red colour, but dissolves scarcely any thing. The fibrin that is not dissolved is a combination of it with an excess of sulphuric acid. By depriving it of this excess of acid, by means of pure water, a neutral combination is obtained which is

soluble in water, and possesses the same characters as the neutral muriate of fibrin.

6. *Nitric acid* of the specific gravity 1.25 digested with fibrin, renders it yellow, and diminishes its cohesion. The fluid becomes yellow, and the surface of the fibrin is covered with a small quantity of fat formed by the action of the acid. During this operation elastic matter is disengaged, which is azotic gas alone, and in which I could not discover the smallest portion of nitrous gas. When the digestion has been continued twenty-four hours, the fibrin is converted into a pulverulent mass, of a pale citron colour, which is deposited at the bottom of the liquor. The latter being decanted off, and the undissolved matter placed on a filter and washed with a large quantity of water, the colour changes in proportion as the acid which was in excess is carried off, and the mass acquires a deep orange colour. Even when the affusion is continued till the water gives no sign of acidity, the orange mass has not yet lost the property of reddening litmus.

This yellow substance was discovered by Messrs. Fourcroy and Vauquelin, who obtained it in treating muscular flesh with nitric acid. They have described it as a new acid formed by the action of nitric acid on the muscular fibre, and to which from its colour they have given the name of yellow acid. (*Acide jaune*). This substance is dissolved in caustic alkali, to which it imparts an orange colour,

and it is partly soluble in acetate of potass and of soda. The French chemists found that if treated with a fresh quantity of nitric acid, it acquires the property of burning with the same phenomena as a combustible body mixed with nitre. This fact they considered as remarkable, since they could not detect any nitric acid in the yellow acid. But the latter, as will be presently shewn, is nothing more than a combination of fibrin with nitric acid, (or in other cases, perhaps, with nitrous acid,) and also with another acid formed by the decomposition of a portion of the fibrin, a species of combination very analogous to the one already described. If the yellow substance be boiled with alcohol, a yellow adipocirous matter is taken up; but it is deposited when the liquor cools. This sebaceous matter has a great resemblance to that obtained by the action of alcohol on pure fibrin. If the yellow substance, after being thus deprived of its adipocirous portion, be digested with water and carbonate of lime, it slowly decomposes the carbonate, disengages its acid in the form of gas, and produces a yellow solution. Having separated this fluid from the undissolved portion of the yellow substance, I concentrated it to the consistence of a syrup: and then poured in alcohol, which precipitated one part, retaining the other in solution. The precipitate had all the characters of malate of lime. Dissolved in water, and decomposed by a sufficient quantity of sulphuric acid, it yielded sulphate of lime; and the acid liquor being filtered and evapo-

rated, gave a brownish and highly acid syrup, possessing all the properties of malic acid. The portion dissolved in the alcohol was evidently a mixture of nitrate and nitrite of lime. The mixture consisting of undecomposed carbonate of lime, and the insoluble part of the yellow substance, being decomposed by diluted muriatic acid, left a yellow mass perfectly similar to the one which I had before decomposed, and possessing, like that substance, the property of reddening litmus paper. It appeared therefore that I had merely exchanged the nitric and malic, for the muriatic acid.

We thus find that fibrin enters into combination as readily with nitric acid, as with the other beforementioned acids, and that it is capable of forming two combinations, the one containing an excess of acid and having a pale yellow colour, the other neutral and of an orange hue. By digesting fibrin with nitric acid, it undergoes a species of decomposition by which malic acid is formed. This acid in conjunction with the nitric acid combines with the undecomposed fibrin. The fibrin thus united with the two acids is certainly in some degree altered, for its neutral combination with nitric acid is insoluble in water, and retains its insolubility and its yellow colour even when the nitric acid has been displaced by muriatic. On the other hand, we have seen that the precipitate thrown down by nitric acid from a solution of fibrin in acetic acid, acquires a yellow tinge, but that water in depriving

it of the excess of acid, renders it gelatinous and again soluble. It follows that the fibrin, which, in the yellow body, performs the office of a saline base to the nitric acid, must be modified in a different manner from what it is in fibrin combined with the acids in the soluble combinations.

The nitric acid, in which the yellow substance has been formed, has a bright yellow colour: it holds in solution a portion of the yellow substance with a quantity of malic acid. Mixed with alkali in excess, it assumes a very dark yellowish brown colour.

7. In *caustic alkali*, fibrin increases in bulk, becomes transparent and gelatinous, and at length is completely dissolved. The solution is yellow with a shade of green. Acids occasion in it a precipitate which gradually becomes confluent. The solution of fibrin in caustic alkali is precipitated by alcohol, which by means of the excess of alkali dissolves a portion of the neutral combination of fibrin with alkali. If the aqueous alkaline solution be evaporated, a coagulum is formed towards the end of the process, probably in proportion as the alkali becomes carbonated. The action of alkali upon fibrin produces some alteration in its properties, for the precipitate thrown down by acetic acid does not dissolve by an additional quantity of acid. But whatever be the nature of this change induced on fibrin by the alkalies, the former is by no means

converted, as M. Fourcroy has alleged, into a fatty substance with which the alkali produces a saponaceous compound, nor has it even the least analogy to any species of soap. As far as I can perceive, this effect of alkalies is confined to the epidermoid textures, and parts which by long and continued boiling are converted into a similar substance.

Of the Colouring Matter and its Chemical Properties.

In order to separate, as much as possible, the colouring matter from the albumen and the salts of serum, I cut the crassamentum into very thin slices which I placed upon blotting paper, till it had taken up all that it could absorb: after which they were dried. A portion of the crassamentum treated in that manner, was triturated with water as long as it appeared capable of acting as a solvent. This water acquired from the matter it had dissolved, a brown colour of so deep a shade that it did not exhibit the least transparency when contained in a glass tube of a quarter of an inch in diameter. The fluid had a faint odour of blood, and a mawkish, saline and highly nauseous taste.

I coagulated the solution by means of heat in a pneumatic apparatus. The mass frothed considerably, but no elastic fluid was disengaged. While yet hot, it was placed on a filter: the liquor had a red colour which it lost by cooling, and at the same time deposited a small quantity of the colouring matter. I shall revert to this liquor in the sequel.

The dark brown coagulated matter, after being carefully washed, and subjected to a powerful press, was dried at a temperature of 70° (centigrade thermometer.) It was but little contracted by the exsiccation, but had become black, hard, difficultly pulverisable, and shewed a vitrous fracture. Before the desiccation is complete, it is of a dark brown colour, has little cohesion, and forms a granulated mass: circumstances by which it may be distinguished from both fibrin and albumen.

1. The colouring matter is acted upon by *boiling water* in nearly the same way as fibrin, becoming somewhat contracted by coction; and the solution contains soda, and an animal matter perfectly analogous to that which is obtained from fibrin, but rather less in quantity: for it would appear that a portion of this substance begins to be formed from the commencement of the process of coagulation. It retains its black colour, but loses the property of softening, and dissolving in acetic acid.

2. *Alcohol* and *ether* convert the colouring matter in part, into a fatty adipocirous mass, having a very disagreeable odour.

3. In *acetic acid*, the colouring matter immediately becomes soft, forming a black and tremulous jelly, which dissolves in tepid water with the disengagement of a small quantity of azotic gas. The solution is reddish brown and only semi-transparent.

A small part of the colouring matter remains undissolved, and forms with the acid a compound of very sparing solubility.

A solution of the colouring matter mixed with acetic acid does not coagulate; but when made to boil turns black and deposits a very small quantity of its insoluble compound, without however coagulating.

The solution of the colouring matter in acetic acid is precipitated both by alkalies and by alkaline prussiates. Ammonia produces a dark brown precipitate, which when well washed and weighed, is found to be the colouring matter unaltered, and again soluble in acetic acid. The solution after precipitation by ammonia is yellow, and deposits by evaporation a quantity of white matter, which is readily distinguished to be albumen, of which it is impossible entirely to deprive the crassamentum.

Prussiate of ammonia precipitates from the acetic solution a mass of a blackish brown colour, resembling the precipitate by pure ammonia. Both these precipitates, employed as pigments, give the same shade of a dirty brown. The prussic acid, therefore, appears to exert no action on the colouring matter of the blood; which should take place if the latter owed its colour to a ferruginous salt. The solution of colouring matter in acetic acid is precipitated by the mineral acids, and the precipitates have pre-

cisely the same characters, excepting the colour, which is brown, as those procured by the same methods from fibrin.

4. Concentrated *muriatic acid* does not dissolve the colouring matter, even when aided by digestion: a small quantity of azotic gas is disengaged and the acid assumes a yellow tint: alkalies, however, produce in it hardly any precipitate. The undissolved portion is a compound with excess of acid which becomes soluble in proportion as the superabundant acid is carried off. The neutral solution of colouring matter is brown, and has the same properties as that formed by acetic acid. The colouring matter boiled for a long time with muriatic acid, suffers a commencement of decomposition; some iron is taken up by the acid, and the undissolved portion is no longer soluble even by repeated washing, although in this state it retains a portion of acid, of which the water cannot deprive it.

5. *The nitric acid* produces the same effects in the colouring matter as on fibrin; the only distinction being in the colour, which in the former is invariably black.

6. *Caustic Ammonia* dissolves the colouring matter assuming a very deep brown colour. A precipitate is thrown down by acids, but not by the alkaline prussiates. The precipitate formed by

acetic acid is again soluble, but only by an excess of acid.

7. In solution of *caustic fixed alkali* the colouring matter is softened, forming a brownish jelly, which is dissolved by a sufficient quantity of water. During the evaporation it coagulates in proportion as the alkali absorbs carbonic acid. The alkaline solution is precipitated by alcohol, which, however, acquires a red tinge by dissolving a small quantity of the compound formed of the colouring matter with the excess of alkali. The alkaline solution of colouring matter seen by day-light has a green colour, but appears red by candle-light. It was upon this appearance of green that Fourcroy conceived the idea that bile might be formed by boiling blood with a small quantity of water; but this green fluid, as we shall afterwards see, agrees with bile in no property excepting colour.

8. If a solution of the colouring matter in water be exposed to a heat of 50° (centigrade thermometer) in a saucer, it blackens and dries completely without coagulating. In this state it is again entirely soluble in cold water.

These experiments prove that the colouring matter has the same chemical properties, and consequently the same chemical composition as fibrin, but that these two bodies are distinguishable from

one another principally by a difference in colour; by the fibrin coagulating spontaneously in all temperatures, while the colouring matter may be dried, without losing its solubility in water, and becomes insoluble only at a certain temperature: and lastly by the peculiar character of the latter when coagulated, such as its not being diminished in volume during the exsiccation, as happens with fibrin.

Inquiry into the influence of the iron contained in the colouring matter, in producing its colour.

The greatest chemical difference that is found between fibrin, albumen, and colouring matter, consists in a quantity of oxide of iron being contained in the ashes of the colouring matter, while none, or at least an infinitely small portion is afforded by the others. Parmentier and Deyeux, to whom we are indebted for an elaborate memoir on the blood, have conjectured that the iron, contained in the blood, was dissolved in a way analogous to the alkaline tincture of Stahl, an opinion which has since been controverted by Fourcroy and Vauquelin, who, on their side, have endeavoured to prove that the colouring matter of the blood was a solution of red subphosphate of iron in albumen. We shall find that neither of these theories can be true, and that the mode in which the iron is combined with the colouring matter will probably long remain unknown.

The colouring matter dried, and exposed to fire in an open crucible, melts and swells up, and at last burns with flame. It leaves behind a porous coal, which cannot be incinerated without the greatest difficulty. For this purpose it must be reduced to a very fine powder, and exposed to the fire in very thin layers. While the charcoal is consuming, it continually exhales a smell of carbonate of ammonia, which proves that the constituents of ammonia are not disengaged from the charcoal by heat alone, but that the influence of oxygen is also required in order to effect the separation. The ashes remaining after the destruction of the carbon is yellow and pulverulent.

The disengagement of ammonia from a mass of burning charcoal which has already been exposed for a long time to fire, is undoubtedly a very remarkable phenomenon; but another and a no less singular fact is presented by the same substance. If the charcoal which has already been long burning, and whose surface is covered with yellow ashes, be pulverised and boiled in nitro-muriatic acid, the acid dissolves the ashes already formed, but does not deprive the remainder of the charcoal of the property of affording a fresh quantity of ashes on re-exposure to the fire. These observations seem to prove that the carbonaceous matter of the colouring substance which remains after the flame has ceased to appear, and after the whole mass has

been subjected to a strong red heat, cannot be, as was before supposed, a mechanical mixture of charcoal with the phosphates and carbonates of the earths and of iron. We must therefore consider it as a chemical compound of carbon, phosphorus, sulphur, with calcium, ammonium, and iron; and it appears that it is in a mode analogous to this combination that the iron as well as the calcium, phosphorus &c. are united with the charcoal, and other constituents of the colouring matter; for it is very evident that the mode in which combustible bodies combine with one another, and with a small portion of oxygen in organic substances, is totally different from that which prevails among the inorganic productions of nature.

To return to the ashes of the colouring matter; water extracts from it a very small quantity of soda mixed with muriate of soda. I have great reason to suspect that these two constituents are merely accidental, and that they appear in consequence of the impossibility of freeing the crassamentum entirely from serum. If the alkaline liquor which is obtained from the ashes, be saturated by acetic acid, evaporated to dryness, and again dissolved, a precipitate will appear on the addition of lime-water, which is phosphate of lime, but in so minute a quantity as not to be appreciable.

I incinerated 20 grammes (400 grains) of colouring matter, till the charcoal was completely destroy-

ed, and obtained 0.25 grammes (3 grains) of ashes having a yellowish red colour. By an accurate analysis, I found it composed of

Oxide of iron - -	50.0
Subphosphate of iron -	7.5
Phosphate of lime with a small quantity of magnesia	6.0
Pure lime - - - -	20.0
Carbonic acid, and loss	16.5
	100.0

But this phosphate of iron was not, in all probability, contained in the colouring matter, not even in the ashes: it has evidently been a product of the analytic process. It is produced in like manner when oxide of iron and phosphate of lime are dissolved together in an acid, and afterwards precipitated with caustic ammonia. Part of the lime remains in solution, and the iron seizes upon its phosphoric acid.

But as one mode of argument will hardly suffice to overturn a theory maintained by chemists of such eminence as Fourcroy and Vauquelin, and for the support of which they have brought forward many positive facts, I have made a number of experiments with a view of throwing light on this subject, and have not met with a single one, which did not appear in contradiction to the opinion of

these celebrated analysts. Some of these experiments I shall now relate.

(A.) The prussiates, as we have already seen, produce no effect on the colouring matter of the blood; and yet they detect, after 24 hours, the least quantity of any ferrugineous salt added to it, having the red oxide of iron for its basis.

(B.) A watery solution of colouring matter, mixed with gallic acid, acquires a beautiful red colour, but the acid produces no precipitate. By adding to the solution of colouring matter one or two drops of a dilute solution of tannin, the liquor becomes of a beautiful red, without any precipitate appearing. But if on the contrary, the solution of tannin be concentrated, it precipitates the colouring matter, and gives it a pale red colour. None of these effects seem to prove the presence of a salt with the base of oxide of iron.

(C.) The aqueous solution of colouring matter, mixed with solution of barytes, is not precipitated: at the end of 24 hours a small quantity of phosphate of barytes is found at the bottom of the vessel, and the liquor has assumed a green colour by the action of the alkaline base. Lime water produces no change.

(D.) A solution of sulphuret of potash produces no change in the solution of colouring matter, ex-

cept that the red colour is slowly converted into green by the action of the alkali. A drop of acetic acid precipitates the sulphur of a white colour.

(E.) It is well known that none of the mineral acids deprive the colouring matter of its colour, which ought to be the case if that colour had been owing to the presence of the subphosphate of iron. The phosphoric acid mixed with the colouring matter blackens without precipitating it: whereas, if the opinion of the French chemists were correct, it ought to produce a neutral and colourless phosphate.

(F.) A drop of a solution of acetate of lead exalts the colour of the colouring matter, and a larger quantity throws down a fine red precipitate.

(G.) The nitrates of silver, of lead, and of mercury blacken the colouring matter, and when added in larger quantity precipitate it.

These experiments prove that the iron in the colouring matter is not contained in it in such a way as to admit of being detected by the best re-agents we possess, until the composition of the colouring matter is totally destroyed.

But how can these facts be reconciled with the following, mentioned by Fourcroy in his *Système des Connoissances Chymiques?* "Vauquelin and I," says

he, "have found that the subphosphate of oxide of iron is easily dissolved in albumen by a slight agitation and without the aid of heat, and that this solution possesses a bright red colour, similar to that of the blood. This colour becomes still more vivid by adding a little caustic alkali, which facilitates still more the solution of the subphosphate in albumen."

It was not till after having made and carefully repeated many experiments, that I could venture to pronounce this opinion to be void of foundation. I shall not seek to form any conjecture with regard to the circumstances which may have deceived these distinguished chemists, but shall merely relate my own experiments on the subject.

The subphosphate in question, newly prepared and still moist, easily mixes with serum, giving it the colour of rust; but it subsides in process of time, and may be entirely separated from it by means of a filter. The neutral phosphate of oxide of iron, mixed with serum also, does not dissolve, and the addition of caustic alkali instantly produces the subphosphate; but this last remains undissolved, and communicates to the liquor a colour of rust, which is quite dissimilar to that of the colouring matter of blood. The serum deprived of subphosphate by filtration retains a pale yellowish colour, which is owing to a small quantity of oxide of iron held in solution.

If serum is mixed with the sub-phosphate in question, and phosphoric acid is added, so as to dissolve the subphosphate, a clear rust-coloured liquor is formed, from which a small addition of caustic alkali precipitates a little albumen, which is again dissolved by a slight excess of the alkali; and then the solution loses its red colour, and the subphosphate falls down, and may be collected on the filter.

In all these experiments the albumen dissolves the oxide of iron, even in greater quantity than exists in the colouring matter of the blood; but this solution is yellowish, and has but little body of colour; and the oxide of iron is shewn by the usual reagents. The albumen of serum will also dissolve many other metallic oxides, for instance, that of copper, as Vauquelin has long since proved; and it is doubtless the albumen which is the menstruum of the oxide of mercury, found dissolved in the blood during a course of this metal for the cure of syphilis. The oxidulum (or black oxide) dissolves easily, and still more copiously, in serum, forming a perfectly limpid sea-green liquor. The solution is readily obtained by adding to serum some salt of iron, which has the black oxide for its base, and neutralizing its acid by an alkali. When this green solution is exposed to the air, it absorbs oxygen, deposits red oxide of iron, and becomes yellow. The compounds of oxide of iron and albumen are decomposed by the mineral acids, which

precipitate the albumen colourless, and retain the iron in solution. The prussiates alone do not disturb the solution of iron in albumen, because the metallic oxide is not here dissolved by any acid; but if, after mixing them, a little muriatic acid is added, an exquisitely beautiful azure blue precipitate appears, consisting of albumen and Prussian blue. If the acid phosphate of iron is dissolved in acetic acid, and afterwards serum be added, followed by a little caustic alkali, the albumen and subphosphate are precipitated together, of a rust-colour, which is not changed by drying, but has no resemblance to the colouring matter of blood. In a word, I have not been able to find any method of combining albumen with subphosphate of iron, or with any other salt of this metal, so as to produce a compound identical with the colouring matter of the blood.

But from the result of all these experiments, what appears to be the difference between venous and arterial blood? This question I am unable to answer; nor can I explain the difference between these substances before and after coagulation.

I have already mentioned that the liquid out of which the colouring matter has been coagulated by heat, has at first a tint of red, which it loses by cooling; at which time it further deposits a small quantity of colouring matter. This liquid, whilst evaporating, becomes green, owing to the action

of the uncombined alkali on the small portion of colouring matter that it still retains. This liquid holds also in solution all the salts, and the soluble animal substances belonging to that portion of the serum which still adheres to the coagulum, and cannot be entirely separated from it. The coagulum, I have found by one analysis, to consist (exclusive of the saline and uncoagulable ingredients) of 64 parts of colouring matter, and 36 parts of an insoluble mixture of fibrin and albumen.

Of the Serum, Albumen, and Salts of the Blood.

When serum is heated in a glass vessel, over a water-bath, it solidifies and forms a pearl-coloured jelly, transparent at the edges. If it is stirred the coagulation is more uniform. It has been said to blacken any silver instrument employed to stir it; but this only happens when the serum has begun to putrefy, or when the bottom of the coagulum has been burnt. As this blackening of silver is owing to sulphur, this substance has also been reckoned among the constituent parts of blood. But it would be equally proper to consider carbon and hydrogen as constituent parts, since these enter into the composition of albumen, in the same way as sulphur does.

Muriatic acid coagulates serum. When heated, a small quantity of azotic gas is evolved. This coagulum has exactly the same properties as the compound of fibrin and muriatic acid.

The sulphuric and nitric acids also produce with the albumen of serum precisely the same compounds as with fibrin.

Phosphoric acid does not coagulate serum.

The acetic acid does not coagulate serum, and when this acid is in sufficient quantity, it prevents the coagulation on heating.

In short, the albumen of serum produces exactly the same compounds with acids and alkalies as the fibrin does, and therefore to avoid repetition, I shall refer the reader back to my observations on this latter substance. The action of alcohol also is perfectly similar in both cases.

There appears therefore to be very little difference between fibrin and albumen; and the latter seems to be intermediate between fibrin and the colouring matter. The only character of distinction between fibrin and albumen is, that albumen does not coagulate spontaneously, but requires a higher temperature for that purpose. Coagulated albumen does indeed dissolve more slowly than fibrin or colouring matter in acetic acid and in ammonia, but probably this is owing to the influence of the heat employed for its coagulation.

Exp. 1. 1000 parts of serum evaporated to perfect dryness, (that is to say, so as easily to be re-

duced to powder,) left ninety-five parts of a yellowish, semi-transparent mass, resembling amber, that split to pieces in drying, which, in curling up, carried with them thick scales of the porcelain glazing of the evaporating vessel.

2. I digested ten grammes of the dried powder in cold water. The albuminous portion softened, and became gelatinous. I separated by the filter the liquid from the insoluble part, and washed the latter repeatedly with boiling water. The undissolved albumen dried in the filter, weighed 6.47 grammes, and did not give up its earthy phosphate, by subsequent digestion in muriatic acid, as this acid remained clear on saturation with an alkali.

3. The solution which had passed the filter, was evaporated to dryness, during which thick membranes formed at the surface, and the solution gelatinized before it was perfectly dry. I digested this residue in alcohol, whilst it was still gelatinous; the spirit assumed a yellow colour, and on evaporation left an alkaline yellowish deliquescent mass, weighing 0.92 grammes. This consisted of soda, holding albumen in solution, of muriate of soda, and muriate of potash, of lactate of soda*, and of an

* In mentioning the *lactate of soda* in this place, I wish to make some observations on the lactic acid, as one of the constituent parts of all animal fluids. It is well known that this acid was discovered by my illustrious countryman, Scheele. Latterly, the

animal matter which always accompanies the lactate.

The portion not dissolved by alcohol and digested with water, left a fresh residue of albumen, weighing 1.95 grammes, and possessing the same characters as that of exp. 2. The watery solution

the French chemists have examined this acid; and Fourcroy, Vauquelin, Thenard, and Bouillon La Grange, have sought to prove that Scheele was mistaken in the supposed peculiar nature of this acid, and that it is only a combination of acetous acid, with some animal matter.

These chemists, however, have made no attempts to obtain this animal matter separate from the acid, and have never succeeded in producing the lactic acid by means of the acetic; but this is the proof which they give of Scheele being in an error. Lactic acid is combined with an alkali, the resulting lactate is distilled with concentrated sulphuric acid, and in the receiver is obtained a mixture of sulphureous, muriatic, and empyreumatic acetic acid, the latter of which is purified, and hence it is, that we are informed that the pretended lactic acid is only the acetous united with some animal matter. But it appears to me, that the French chemists have only cut the Gordian knot; for one of the properties of the lactic acid, is to be incapable of volatilization, and it is a property of the sulphuric acid to change many organic substances with which it is distilled, into the empyreumatic acetous and sulphureous acids. By a parity of reasoning, almost every one of the freed vegetable acids might be proved to be only acetic acid, combined with some matter which deprives it of its volatility, without destroying its other acid properties; and in fact it is thus, that Bouillon La Grange has inferred that the malic and gallic acids are only varieties of the acetic. In an analysis of muscular flesh, which I made in the year 1806, I found that the humours of the muscles contained a free acid, which, by many experiments, I discovered to have all the properties which

could not be made to gelatinize, and did not hold the smallest quantity of gelatine. Besides the alkali, it contained an animal matter easily precipitable by tannin and by muriate of mercury, and which appeared to me to be extracted from the al-

Scheele attributes to the lactic acid. I collected a quantity large enough for examination, and I succeeded by different methods, in obtaining it in greater purity than Scheele had procured his. I examined a great number of its saline combinations, with alkalies, earths, and metallic oxyds, the particulars of which are given at length in the second volume of my *Treatise on Animal Chemistry, Stockholm,* 1808. *p.* 430. *& seq.* If therefore it is allowed, that two acids that produce saline compounds different from each other, cannot be identical in their nature, the distinction between the lactic and the acetic acids cannot be controverted. It is since that time that I have discovered the lactic acid, free or combined, in all animal fluids.

Many chemists have observed, in their analysis of animal fluids, that the alcoholic solution when evaporated leaves a yellow, deliquescent extractive mass. Of late, more attention has been paid to this extract, and if I mistake not, it has been considered as a single substance, and has received the name of *osmazome.* One of the component parts of this extract is *lactate of soda,* and the other, with which it is intimately combined, is an animal matter that may be separated by means of tannin. To prove the presence of lactic acid, dissolve the whole in alcohol, and add a mixture of sulphuric acid much diluted with alcohol, as long as there appears any precipitate, which is sulphate of potash or soda. Digest this spirituous solution (which contains muriatic, sulphuric, lactic, and sometimes phosphoric acid) with carbonate of lead, and all the above acids will unite with the oxyd of lead, but of these only the lactate will be soluble in alcohol. Decant the alcoholic solution of lactate of lead, separate the lead by a stream of sulphuretted hydrogen gas, and by evaporating the clear liquor, the lactic acid will remain in the state of an acid syrup.

bumen by the boiling water, during its coagulation, and to be analogous to the substance obtained by boiling fibrin with water.

We have been told, that the blood contains much alkaline and earthy phosphates. I coagulated a large quantity of serum, and thus procured a good deal of the residuary uncoagulable fluid. This I mixed with barytic water, which after a time gave a slight precipitate, soluble in muriatic acid. Some of the same serum, mixed with lime-water, was not clouded. It follows from this, that the blood contains no sulphuric acid, and only a vestige of the phosphoric. In my Treatise on Animal Chemistry, I have endeavoured to prove, that the phosphates, as well as the lactates, are always produced by the spontaneous decomposition of animal substances, and that the small quantity of each which is found in the blood, is carried thither by the absorbent system, in its progress to the secretions, through which it is discharged from the body, and hence the secretions contain always a much larger proportion of these acids.

Not to be too diffuse, I shall pass over the description of the methods I employed, to ascertain the respective proportions of the contents of the serum, and shall only give the results:

A thousand parts of serum, I find to consist of

Water - - - -	905.00
Albumen - - - -	79.99

Substances soluble in alcohol, viz.		
Lactate of Soda, and extractive matter	6.175	8.74
Muriate of soda and potash	2.565	
Soda and animal matter soluble only in water		1.52
Loss		4.75
		1000.00

I washed the albumen well in this experiment, digested it in muriatic acid, and then burnt it to ashes, which were almost exactly equal in quantity to the ashes produced by the combustion of the same weight of colouring matter. But the ash of the albumen was white, and did not shew a particle of iron. I found a trace of soda, but the greatest part was phosphate and carbonate of lime, with a little magnesia. It is clear, therefore, that the earthy salts found in the ashes of coloured blood had not been dissolved in the blood, nor even existed as salts in the blood, from which they were obtained by means of combustion. Hence we may conceive, how the blood can produce and deposit in the animal economy, the earthy phosphates, which, however, are not soluble either in pure water, or in the blood; and hence too we may infer, that the production of bone cannot be considered as a simple crystallization of a salt, conveyed by the blood in a state of solution, but re-

quires us to suppose the decomposition of the animal matter of the blood, as well as in any other secretion.

All the authors who have written on the blood, assert that gelatine is one of its component parts. This, however, is a mistake, and arises from the gelatinous appearance of the albumen, as I have never been able to detect a particle of gelatine in blood; and, as far as my researches extend, I have found gelatine to be a substance altogether unknown to the economy of the living body, and to be produced by the action of boiling water on cartilage, skin, and cellular membrane, substances which are totally distinct from fibrin and albumen.

On Human Blood.

The blood of man perfectly resembles in composition that of the ox, but the coagulum of human blood is more easily decomposed by water, and the fibrin thus obtained is more transparent. When dried, it amounts to no more than 0.75 from 1000. parts. Human fibrin has the same chemical properties with that of the ox, but is more readily incinerated: the white ash consists of the phosphates of lime and magnesia, a little carbonate of lime, and soda.

The colouring matter of human blood is also chemically the same with that of ox blood, but is much more easily reducible by fire to the same yellow ash, which seems to shew that it contains

less azote or ammonium. A hundred parts of dried colouring matter of human blood, gave 15. parts of ash, of which 3 parts dissolved in water, and was alkaline, and when saturated with acetic acid, and mixed with muriate of barytes, it left a copious precipitate of phosphate of barytes, soluble in an excess of muriatic acid. I found in this acetic solution, no trace either of muriatic acid or of potash. It appears therefore, that soda and phosphoric acid as well as the earthy phosphates, are products of the combustion. As to the portion of the ash of colouring matter, which was insoluble in water, it consisted of the same substances in nature and in proportion, as that of the ash of the colouring matter of ox blood,

The serum of human blood is composed (according to my experiments) of

Water - - - - -		905.0
Albumen - - - - -		80.0
Substances soluble in alcohol, *viz.*		
Muriate of potash and soda	6.	10.0
Lactate of soda, united with animal matter	4.	
Substances soluble only in water, *viz.*		
Soda, phosphate of soda, and a little animal matter -		4.1
		999.1*

* I cannot refrain here from comparing my analysis, with that made in this country by Dr. Marcet, and given in the second Vol. of these Transactions, page 370.

Dr.

Human albumen is more easily incinerated than that of the ox, and contains more soda and phosphate of soda. A hundred parts of the dried albumen give twelve parts of calcined ash.

The muriates found in human blood are triple the quantity of those in ox blood, owing doubtless to the salt consumed by man in his food. Human blood also contains a larger proportion of muriate of potash.

On the whole, the great agreement in the composition of human and of ox blood is remarkable, and explains to us the possibility of the phenomena observed in the experiments in transfusion.

General results of the analysis of Blood.

1. Blood is composed of one portion, which is

Dr. Marcet finds the following ingredients;

Water	900.0
Albumen	86.8
Muriate of potash and soda	6.6
Muco-extractive matter	4.0
Sub-carbonate of soda	1.65
Sulphate of potash	0.35
Earthy phosphates	0.60

A more perfect agreement cannot be expected in the analysis of substances liable to so many accidental differences, particularly in the quantity of water, which in the blood depends so much on the proportion of liquids taken into the stomach. It is clear that Dr. Marcet's *extractive matter* is impure lactate of soda; and I must also observe, that the sulphate of potash and the earthy phosphates found by him in the ashes of serum, are probably, for the reasons above-mentioned, formed by the process of combustion.

liquid and homogeneous, and of another which is only suspended and spontaneously separates when at rest.

2. The liquid part is a solution of much albumen and a little fibrin, both combined with soda. It also contains some other saline and animal substances, but in very small quantity.

3. The portion which is suspended is the colouring matter. It differs from the albumen chiefly in its colour, and its insolubility in serum. The colour seems to be owing to iron, of which it contains $\frac{1}{3}$ per cent of its weight, but which cannot be separated from it as long as it continues to be colouring matter. This separation can only be affected by combustion, or by the concentrated acids, both of which agents entirely decompose the substance with which the metal was combined. The colouring matter cannot be artificially produced by uniting albumen with red sub-phosphate of iron.

4. Fibrin, albumen, and colouring matter, resemble each other so closely, that they may be considered as modifications of one and the same substance*. I shall in future call them *albuminous*

* One of the most striking points of difference, exists in the property which the colouring matter has of absorbing oxygen, and thereby experiencing a very remarkable change of colour. Serum absorbs very little oxygen, and only in proportion as it is decomposed. Can the iron in the colouring matter give it this property? This is probable; but we shall never arrive at any accurate knowledge of these phenomena, without first analyzing these

contents of the blood, when speaking of them collectively. These three substances *produce* when decomposed, but *do not contain* earthy phosphates and carbonate of lime; and indeed, the entire blood contains in solution no earthy phosphate, except perhaps in too small a quantity to be detected.

5. The albuminous contents of the blood will unite with acids, and produce compounds, that may be termed saline; these, when neutralized, will dissolve in water, but separate on adding an excess of acid. The acetous and phosphoric acids however must be excepted, as an excess of either of these forms a compound soluble in water. Nitric acid, digested with the albuminous contents, forms an insoluble compound consisting of the albumen in an altered state, and of the nitric and the malic acids. This property of combining with acids, is retained in some instances by the albumen after it has undergone the changes produced in the secretory organs; as for instance in the peculiar matter of the bile, the curd of milk, &c.

6. The blood contains no gelatine*.

these elements of the animal kingdom with the most scrupulous exactness. It is then, and not till then, that we may form conjectures; at present they are useless.

* It gives me great pleasure to find that an English chemist, Dr. Bostock, has arrived at the same conclusion, without any previous knowledge of my work, and (owing to a delay in the publication) prior to its appearance. See Dr. Bostock's experiments, published in the first Volume of these Transactions.

Secreted Fluids.

There exists no problem in chemistry more difficult to solve, than that of the secretion of animal fluids. The circulating fluid is carried to the organized laboratory which nature employs, no foreign ingredient is added, no chemical reagent is interposed, and yet the fluid which flows from these organs has acquired chemical properties, which render it decidedly different from the common circulating mass. Not only is the chemical agent which produces these changes unknown to us, but we shall in vain search for any analogous chemical operation. It is doubtless easy to conjecture, that it is by the influence of the nervous system, that this decomposition of blood into the secreted fluids is effected; but what is this influence? If electric, how can it be brought to accord with our present knowledge of electric agency? But avoiding vain conjectures on a subject which perhaps will ever remain a mystery, let us determine from the knowledge we already possess, the chemical nature of the materials of their products. In proportion as we acquire light on the nature of the former, the analysis of the latter becomes more and more interesting, and much may be done by a judicious comparison of the one with the other.

There are two classes of secreted fluids; namely, *the secretions*, properly so called, or the fluids intended to fulfil some ulterior purpose in the ani-

mal economy; and the *excretions* which are directly discharged from the body. The fluids of the former class are all alkaline; and of the latter, all acid. The excretions are the urine, the perspired fluid, and the milk. All the other fluids appear to belong to the former class.

The alkaline secreted fluids may be divided into two very distinct species. The former of these contains the same quantity of water as the blood, so that the change induced by the nervous influence, seems to be confined to that of altering the chemical form of the albuminous materials, without affecting their relative proportion to the water and other substances dissolved in the blood. The bile, spermatic fluid, &c. are of this kind. The latter species consists of fluids in which the influence of the nervous system has separated a large portion of the albuminous matter, and left the remaining liquid proportionally more watery. The saliva, the humors of the eye, and the effused serum of membranes are of this species; and in these the quantity of salts, and in general also of alkali, is the same as in the blood.

The influence of the chemical agent of secretion is therefore chiefly spent upon the albuminous materials of the blood, which seems to be the source of every substance that peculiarly characterizes each secretion, each of which is *sui generis*, and is its principal constituent. All the other parts of the

secretion seem to be rather accidental, and to be found there only because they were contained in the blood out of which the secretion was formed.

Therefore in examining the secreted fluids the chief attention should be paid to the peculiar matter of the fluid, which varies in all. This matter sometimes retains some of the properties of albumen, at other times, none; and hence an accurate analysis shewing the quantity and nature of this peculiar matter is above all to be desired.

If the several secretions be supposed to be deprived of their peculiar matter, and the remainders analyzed, the same residue would be found from them all, which also would be identical with the fluid separated from the serum after its coagulation. Thus we should find, first, a portion soluble in alcohol, consisting, as has been already shewn, of the muriates of potash and soda, of lactate of soda, and of an extractive animal substance precipitable by tannin: and secondly, of a portion soluble only in water, containing soda (which acquires carbonic acid by evaporation, and is separable by acetic acid and alcohol) and another animal substance, not extract, precipitable from its solution in cold water both by tannin and by muriate of mercury. Sometimes a vestige of phosphate of soda will also be detected.

The excretions are of a more compound nature.

They all contain a free acid, which is the *lactic*, and in the urine this is mixed with the uric acid. Urine seems to contain only a single peculiar characteristic matter; but milk has as many as three, namely, butter, curd, and sugar of milk, which, however, seem to be produced by different organs that mingle their fluids in the same receptacle. The perspired fluid appears to have no peculiar matter, but to be a very watery liquid with hardly a vestige of the albumen of the blood, and, in short, is the same as the other excretory fluids would be when deprived of their peculiar matter. If we suppose this matter taken away from those excretions that possess it, the remaining fluid will be found to have properties very different from the fluid part of the secretions, when equally freed from their peculiar matter. That of the excretions is acid, contains earthy phosphates, and when evaporated, leaves a much larger residue than the fluid of the secretions. This residue is yellowish-brown, of the consistence of syrup, with an unpleasant sharp saline taste of the salts that it contains. It reddens litmus, is mostly soluble in alcohol, and this spirituous solution contains the muriates of the blood together with free lactic acid, much lactate of soda (the soda being the free alkali of the blood neutralized by this acid) and the extractive matter which always accompanies this neutral salt. The part insoluble in alcohol contains a distinguishable quantity of phosphate of soda, a little of a similar animal matter to that found in the secretions, and

also the earthy phosphates which were held in solution by the lactic acid, and were precipitated by the action of the alcohol. The urine possesses also a number of other substances, which will be specified when describing this excretion in particular.

Having thus given some general views of the composition of all the secreted fluids, I shall proceed to give a short account of each individually.

Of Bile.

It is well known that the elder chemists considered the bile as an animal soap composed of soda and a resin. The accuracy of this opinion had often been questioned, owing to the very small proportion of soda; and lately our skilful contemporary Thenard, has published an analysis of bile, in which he gives as its component parts, soda, a peculiar matter named by him *Pieromel* and a resin, which united, produce a fluid that has the taste and other distinguishing properties of this secretion. Nevertheless I am convinced that there is no such resin as Thenard and his predecessors have described. I shall not here relate my experiments on this supposed resin in particular, but shall give the results of my inquiries on the bile itself, which will enable the reader to confirm or reject my opinions according as he finds them founded on accurate experiment.

The substance which is peculiar to bile has an

excessively bitter taste followed by some sweetness; the smell is also peculiar, and the colour in most animals varies from green to greenish yellow. It is soluble in water, and its solubility is not in the least promoted by the alkali of bile, since, when this is neutralized by any acid, the peculiar matter does not separate: it also dissolves in alcohol in all proportions. Like the albuminous materials of the blood of which this peculiar matter is composed, it will unite with acids, producing compounds of two degrees of saturation, and hence, of solubility. The acetous acid, which gives soluble compounds with the albumen of the blood, does the same with the peculiar matter of the bile; and hence this matter is not precipitated on adding this acid to bile, though it falls down on the addition of the sulphuric, nitric, or muriatic acids. It is this sparingly soluble compound of biliary matter with a mineral acid which has been mistaken by many chemists for a resin; since it possesses the external characters of a resin, melts when heated, dissolves in spirit of wine, and is again precipitated (in part at least) by the addition of water. The alkalies, alkaline earths, and alkaline acetates decompose and dissolve it: the former by depriving it of its combined acid; the latter, by furnishing it with acetous acid which renders it soluble in water.

The peculiar matter of bile will also combine with many metallic oxides into a pulverulent mass; and the above described resiniform compound of this

matter and any of the mineral acids often forms with the same oxides a substance like a plaster, resembling in this respect also the true resins.

The degree of insolubility possessed by these compounds of acid and biliary albumen varies both according to the species of animals, and also according to the length of time that the bile has been extracted; for the longer it has been kept, the more solubility these compounds acquire; but in this case I have always found that by pouring in a fresh quantity of acid, and slowly evaporating the mixture, the resinous matter falls down as the supernatant liquor becomes more acid.

The biliary matter may be obtained pure in the following way: mix fresh bile with sulphuric acid diluted with 3 or 4 times its weight of water; a yellow precipitate of a peculiar nature first appears, which must be allowed to subside and be removed; then continue to add fresh acid as long as any precipitate is formed; heat the mixture gently for some hours, and afterwards decant the fluid part, and thoroughly edulcorate the green resin which is left. This resin reddens lítmus, and is partially and sparingly soluble in water. It may be deprived of its acid in two ways: one of them is by digesting it with carbonate of barytes and water, whereby the carbonate is decomposed, and the water forms a green solution possessing all the peculiar characters of bile: the other way is by dissolving it in alcohol

and digesting the solution, either with carbonate of potass or carbonate of lime till it no longer reddens litmus, and then evaporating it to dryness. Either of these methods will give the pure biliary matter, and there are also other ways of obtaining it, which I have described in my work on Animal Chemistry, Vol. II. p. 47.

This peculiar biliary matter when pure, resembles exactly entire desiccated bile. Being soluble in alcohol it might be supposed that it would dissolve in ether, but this is not the case, for ether only changes it to a very fetid adipocirous substance, exactly as it acts upon the albuminous matter of the blood. One circumstance relating to the biliary matter has much surprised me, which is, that it gives no ammonia by destructive distillation. Therefore it contains no azote; but what can have become of the azote of the albuminous matter of the blood? for, no vestige of azote is found in any other of the constituent parts of the bile, nor does bile contain any ammonia.

The following is the result of my analysis of bile.

Water - - - - - - - -	907.4
Biliary matter - - - - -	80.0
Mucus of the gall-bladder, dissolved in the bile - - -	3.0
Alkalies and salts (common to all secreted fluids) - - - -	9.6
	1000.0

2. *The Saliva.*

The saliva is one of the fluid secretions which contain more water than the blood. When first discharged from the mouth it holds suspended a mucus which is not dissolved in the saliva, but gives it its frothy quality. This mucus gradually subsides from the saliva when kept in a cylindrical vessel, and with more ease when previously diluted, after which the supernatant saliva may be decanted off.

Saliva is composed of

Water - - - - - -	992.9
A peculiar animal matter - -	2.9
Mucus - - - - -	1.4
Alkaline muriates - - -	1.7
Lactate of soda and animal matter	0.9
Pure soda - - - - -	0.2
	1000.0

Two of these ingredients require further notice.

The peculiar animal matter of the saliva is obtained by adding spirit of wine to dried saliva, which dissolves the muriates, lactates, &c. The soda which remains in the insoluble portion is then extracted by fresh spirits slightly acidulated with acetous acid. The residue is mucus mixed with the peculiar salivary matter, which last may be dissolved by water, leaving behind the insoluble mucus.

This peculiar matter is therefore soluble in water but not in spirits of wine. The solution in water, when evaporated to dryness, leaves a transparent mass that easily again dissolves in cold water. This solution is not precipitated either by alkalies, or acids, or subacetate of lead, or muriate of mercury, or tannin; neither does it become turbid by boiling.

The *mucus* of the saliva is readily procured by mixing saliva with distilled water, from which the mucus gradually subsides, and it may then be collected on the filter and washed. In this state it is white, and would seem to contain phosphate of lime mixed with it. This mucus is quite insoluble in water; it becomes transparent and horny in the acetic, sulphuric, and muriatic acids, but does not dissolve in them, and the alkalies separate nothing from them. The mucus therefore contains no earthy phosphate, though its appearance would lead to suspect this earthy salt. It dissolves in caustic alkali, and is again separated from it by the acids. A small proportion escapes the action of the alkali, but yields to muriatic acid, and is not separable from this acid by an excess of alkali. The mucus of the saliva is very easily incinerated, and though no phosphate of lime is detected in it by the acids in its natural state, a considerable portion of phosphate appears in the ash after combustion.

Is this mucus secreted in the salivary glands, or is it only the common mucus of the mouth? The latter appears most probable, though I confess that the large quantity of this mucus contained in the saliva, and the great difference between its chemical properties and those of the nasal mucus, throw some doubt on this opinion.

It is this mucus that produces the tartar of the teeth, which at first is only mucus precipitated on the surface of the teeth and adhering to them, but soon it begins to decompose, its colour changes by the influence of the air from white to yellow or greenish, the warmth and moisture of the mouth contribute to complete the decomposition, and the same earthy phosphates which are produced by oxidation and combustion in open fire are here formed and slowly deposited on the surface of the tooth by a slower but a similar process. The tartar is therefore as it were the *ash* of mucus crystallized on the tooth, and this, as is well known, will in length of time form very large incrustations.

I have found it to consist of the following substances:

Earthy phosphates	79.0
Mucus not yet decomposed	12.5
Peculiar salivary matter	1.0
Animal matter soluble in muriatic acid	7.5
	100.0

8. *The Mucus of the mucous membranes.*

I shall premise some remarks on the term *mucus* as applied to animal chemistry. It properly signifies the mucus of the nostrils, but many chemists have extended it to other substances found in the animal fluids, so that Jordan, Bostock, Haldat and others reckon it among the constituents of these fluids. None of these chemists has considered mucus, used as a general term, as identical with the nasal mucus, or, if they have thought so, it has been a very great error. I must now mention that there is no such principle as the mucus of animal fluids, the substance so considered being in reality lactate of soda mixed with the animal matter that always accompanies it. But if it did exist as a separate principle, some other term should have been used, to distinguish it from the mucus of the nostrils which is very different.

The chemists who have the most attended to the analysis of mucus, have been Messrs. Bostock, Fourcroy, and Vauquelin, but none has given a very satisfactory account of its properties. The two latter chemists, who have published a long memoir on animal mucus, have too much generalized the characters peculiar to nasal mucus, in attempting to extend them to the mucus of the intestines and gall bladder, for example, where they are totally inapplicable.

The mucus of mucous membranes is produced

from the same secretory organ throughout the body, and possesses everywhere the same external characters which constitute *mucus*; but in chemical properties the mucus of different organs varies considerably according to the required use in protecting these organs from the contact of foreign substances. Thus the mucus of the nostrils and trachea, which is intended to protect these membranes from the external air, differs from that of the urinary bladder which is to preserve this organ from the contact of an acid liquor, or from that of the gall-bladder whose contents are alkaline.

The animal matter peculiar to mucus is the same in all cases, and has the following properties: it is insoluble in water, but is able to imbibe so much of this fluid as to become more or less transparent, semi-liquid or what is termed *glairy*. If in this state it be laid on blotting paper, and the paper changed as it becomes wet, the mucus may be deprived of the greater part of the moisture which it had absorbed, and will then lose most of its peculiar properties. Mucus is not coagulable by boiling, it becomes transparent when dry, and generally resumes its mucous character on adding fresh water, but there is great difference in this property.

The liquid part of mucus, or that fluid which the proper mucous matter imbibes, and to which it owes its fluidity, is the same as the liquid that remains after the coagulation of the serum.

My experiments give the following constituent parts of nasal mucus:

Water - - - - - -	933.7
Mucous matter - - - - -	53.3
Muriate of potass and soda -	5.6
Lactate of soda with its accompanying animal matter - - -	3.0
Soda - - - - - - -	0.9
Albumen and animal matter insoluble in alcohol, but soluble in water; along with a trace of phosphate of soda - - - - -	3.5
	1000.0

Nasal mucus when just secreted contains a greater proportion of water than above stated. It is very fluid, and gives by evaporation only 0.25 per cent. of solid matter. There is reason to suppose that its peculiar animal matter is first dissolved in the free alkali, but is gradually precipitated as the alkali becomes carbonated by the contact of the respired air. The mucus which I analyzed was of such a consistence that the whole quantity fell out on inclining the vessel that contained it.

The proper mucous matter of the nose has the following properties: immersed in water it imbibes so much moisture as to become transparent, excepting a few particles that remain opake: it may then be separated by the filter from the rest of the water,

and may be further dried on blotting paper till it has again lost nearly the whole of the moisture it had imbibed. Mucus thus dried will again absorb water when immersed in it, and resume its transparency; and this alternate wetting and drying may be repeated an indefinite number of times, but it thus gradually becomes yellowish and more resembling pus. Five parts of recent mucus absorbed by 95 parts of water produce a glairy mass which will not pour from a vessel. When mucus is boiled with water it does not become horny, nor does it coagulate; the violent motion of ebullition rends it in pieces, but when the boiling is discontinued, it is found collected again at the bottom of the vessel, and nearly as mucous as before. I should observe, however, that this mucus naturally contains a little albumen, which must first be extracted by cold water to enable the remaining mucus to exhibit the above mentioned appearances. The nasal mucous matter dissolves in diluted sulphuric acid: when the acid is concentrated, the mucus is carbonized. Nitric acid at first coagulates it, a number of yellow spots being dispersed through the coagulum; but by continuing the digestion it softens and is finally dissolved into a clear yellow liquid containing none of that yellow substance which I have described under fibrin.

Acetous acid hardens mucous matter, but without dissolving it, even in a boiling heat. Caustic alkali at first renders mucous matter more viscous, and afterwards dissolves it into a limpid flowing

liquid. Tannin coagulates mucus both when softened by the absorption of water, and when dissolved either in an acid or an alkali.

The mucus of the Trachea, as far as I have been able to examine, possesses the same properties with the preceding. The first morning expectoration often contains bluish or dark coloured flocculi, which will imbibe 20 times their bulk of water, and sometimes become thereby so perfectly transparent as hardly to be distinguished in the surrounding water. Acids and alkalies act upon them as on nasal mucus.

The *mucus of the Gall-bladder* much resembles that of the nostrils, but is more transparent, and is always tinged yellow by the bile. When dried it will again soften in water, but loses part of its mucous property. Biliary mucus dissolves in alkali, and its fluidity increases in proportion to the quantity of the latter. If this solution is exactly saturated with an acid, the mixture becomes slightly turbid and of a consistence to be drawn out in threads. All the acids produce with biliary mucus a yellowish coagulum that reddens litmus. The coagulum formed with the sulphuric acid may be restored to its mucous properties by exact saturation with an alkali. Alcohol coagulates this mucus into a very yellow granular mass, to which the mucous property cannot be restored. A similar mass is often found in the adipocirous biliary concretions, and it is re-

markable that it may be produced from mucus by alcohol, as from biliary matter by ether.

Bile itself is often of such a mucous consistence as to be drawn out in threads. This is owing to the presence of mucus dissolved in the alkali of the bile. A very little of any acid (the acetous for example) precipitates the mucus, and destroys the viscosity of the bile. Alcohol has the same effect.

Former chemists seem to have regarded this property of bile as owing to the presence of albumen, the existence of which has been considered as demonstrated by the precipitate caused by adding acetic acid or alcohol. From what I have already mentioned, it is clear that no precipitate produced by acetic acid can be albumen, since the latter is soluble in this acid; and as bile is not disturbed by prussiate of potass or by tannin after the precipitate by acetous acid is removed, this is a proof that no albumen can be contained in bile. The following experiment is conclusive that the supposed albumen of bile is only mucus: mix some bile with very weak sulphuric acid, drain on a filter the yellow precipitate thus formed, and then digest it with a saturating quantity of carbonate of soda and water, and the precipitate will be changed to a mucus which will be more or less glairy according to the quantity of water employed.

The *mucus of the Intestines* accompanies the ex-

crements, in which it often forms long and transparent filaments. When once dried, the addition of water will not restore its mucous property, but alkalies produce this effect, though without rendering it transparent.

The *mucus of the Urinary Passages* accompanies the urine, in which it is partly dissolved and partly suspended mechanically. The latter portion is generally too transparent to be distinguished by the eye, but it may be exhibited by letting the urine remain awhile at rest, decanting the fluid portion, and drying the mucus on a filter. This loses its mucous property totally by desiccation, and then often becomes rose-coloured, owing to the presence of uric acid, and appears to be crystallized. It softens a little in water. The urinary mucus is easily soluble in alkalies, and is not separated from this solution by acids. Tannin separates it in white flocculi. I shall return to this subject under the analysis of urine.

4. Fluids of the Serous Membranes.

It is well known that the surface of serous membranes is always moistened by a liquid, which in a state of health is never secreted in quantities sufficient for analysis; it is therefore only during a dropsical state of these membranes that we can gain any knowledge of its properties. This fluid may be considered as serum deprived of from $\frac{2}{3}$ to $\frac{4}{5}$ of its albumen. It does not coagulate by mere boiling,

but it gradually becomes turbid, and during the evaporation a coagulated mass collects. This appears to be albumen, but it has a sulphur-yellow colour. It is composed of

Water	988.30
Albumen	1.66
Muriate of potass and soda	7.09
Lactate of soda and its animal matter	2.32
Soda	0.28
Animal matter only soluble in water, with a trace of phosphates	0.35
	1000.00

The fluid whose analysis is here given was that of Hydrocephalus*, which probably approaches

* It gives me much pleasure again to fall in the track of Dr. Marcet's labours, who has analyzed many of these fluids with results so nearly approaching my own as to be a considerable confirmation of their accuracy, particularly as our experiments were made nearly at the same time, and without any knowledge of each other's operations. The following are Dr. Marcet's results.

	Fluid of Spina bifida.	*Fluid of Hydrocephalus internus.*
Water	988.60	990.80
Muco-extractive matter, &c.	2.20	1.12
Muriates, &c.	7.65	6.64
Subcarbonate of soda	1.35	1.24
Phosphates, &c.	0.20	0.20

I may observe that the circumstance of Dr. Marcet's having found a greater quantity of soda is owing to the decomposition of the lactate, as well as to the presence of carbonic acid.

nearer than any other of the morbid effused fluids to the natural state, on account of the short duration of the disease and the little time to which the fluid is exposed to spontaneous change within the ventricles of the brain. The other dropsical fluids are in general more concentrated, which may arise either from the mere consequence of being long kept, or from the transudation of the serum of the blood that always occurs in the last stages of dropsy, and appears also to take place into the urine and cellular membrane.

5. *The Humors of the Eye.*

The quantity of these fluids that can be procured is so small that it is not easy to obtain a very exact analysis of them. However, my experiments have shewn me that they bear a very close affinity with the other membranous fluids. Those of the eye are distinguished by being perfectly transparent and colourless; the other membranous fluids having a yellowish tinge. The humors of the eye are not coagulated by boiling. Their composition is as follows:

	Aqueous Humor.	*Vitreous Humor.*
Water - -	98.10	98.40
Albumen, - -	a trace	0.16
Muriates and lactates	1.15	1.42
Soda, with animal matter soluble only in water	0.75	0.02

The crystalline lens has a peculiar and very remarkable composition. It has been considered as a muscle from the well-known experiment of M. Reil, who, on treating it with nitric acid, discovered in it a peculiar muscular structure; and Mr. Chenevix also found that its density and specific gravity increased towards the center. But its solubility in water is a sufficient proof that it is not a muscle, though to effect this solution it is necessary to break it down, and then it leaves undissolved a small portion of extremely pellucid membrane. This circumstance, added to that of the increasing density towards its center, shews that the structure of the lens is cellular, the cells being filled with pellucid matter of different degrees of concentration.

The composition of the lens I have found to be as follows:

Water		58.0
Peculiar matter		35.9
Muriates, lactates, and animal matter, all soluble in alcohol	2.4	
Animal matter soluble only in water, with some phosphates	1.3	
Portions of the remaining insoluble cellular membrane	2.4	
		100.0

The matter peculiar to the lens is remarkable. It coagulates by boiling, and the coagulum has all

the chemical properties of the colouring matter of blood, except colour, which is entirely absent. When burnt, it leaves a little ash containing a very small portion of iron. The liquor in which the coagulum is formed reddens litmus, has the smell of the humors of the muscles, and like them contains free lactic acid.

The perfect achromatic transparence of the lens, notwithstanding its similarity in chemical properties to the colouring matter of the blood, is well worthy of notice. The black pigment of the choroidea is a powder insoluble in water and acids, but slightly soluble in alkalies. When dried and ignited, it burns as easily as a vegetable substance, and the ash contains much iron. From these observations it may well be supposed that the circulating blood is decomposed on the interior surface of the choroidea, leaving there its colouring matter, which is required for the purposes of vision, and conveying the remainder to the inner part of the eye perfectly limpid and colourless. Need I add that the received opinion of the presence of gelatine and albumen in the lens is erroneous? The existence of free lactic acid in the humors of the lens proves nothing with regard to its supposed muscular structure; but only shews the presence of absorbing vessels to convey the products of the spontaneous decomposition of animal matter, one of the most important of which appears to be the lactic acid.

Of the Fluids that compose the Excretions.

I must refer the reader to what I have already observed generally on this subject, and shall proceed to the individual species.

1. *The Fluid of Perspiration.*

I have not been able to make my experiments on this fluid in any large quantity, the organ that secretes it being extended over so large a surface and seldom affording much at a time. As to what collects in under-waistcoats after some days wearing, it must always be more or less altered by spontaneous decomposition.

I collected on a watch glass a few drops of sweat as they fell from my face, and evaporated them carefully. The yellowish residue had all the appearance under the microscope of the usual mixture of the muriates of potass and soda with lactic acid, lactate of soda, and its accompanying animal matter. It reddened litmus, and dissolved in alcohol, and was without doubt of the same nature as the analogous matter found in the other fluids. The alcohol left untouched a small trace of an animal matter which blackened in the fire, but was in too small quantity to admit of further examination.

Mr. Thenard discovers acetous acid in perspiration, but this acetous acid is produced here, as in his other experiments, from the lactic, by his mode

of operating. It is well known that litmus paper reddens instantly when put in contact with the skin of the living body; which shews that the acid that produces this effect is not volatile, otherwise it would be evaporated by the dry surface of the body which has always a temperature of 86° to 90° *Fahr.*

2. *Urine.*

We possess many analyses of urine, both in a healthy and diseased state, but none of them gives a very extended view of the subject; and it is clear that the analysis of morbid urine acquires its chief interest from being able to compare it with that of health.

A. *The acids of the Urine.*—The acidity of healthy urine has generally been attributed to the phosphoric acid. By the chemical change which the blood undergoes in the kidneys, a large portion of its constituent parts are acidified; so that the blood which enters alkaline into the renal arteries, returns from the renal vessels loaded with many acids, some of which did not at all exist in the blood at its entrance; and others were present in very minute quantity only. The acids of the urine which do not at all exist in the blood are the sulphuric, uric, and sometimes the benzoic; the others are the phosphoric and lactic. The muriatic and fluoric acids appear to pass from the blood to the urine without increase in their proportional quantity. As by the laws of chemical affinity these acids will unite with

any alkali that may be present, and saturate themselves with it in the order of the force of their respective affinities, it must follow that where the quantity of alkali is insufficient to saturate all the acids present, the weakest acids must be those that will remain uncombined and will give the urine its acid properties. These therefore must be the lactic and the uric.

It is so generally known and so fully proved that the urine contains the *phosphoric*, *muriatic*, and *uric* acids, that it is useless to add any thing further on this subject.

Urine contains the *fluoric* acid. In my analysis of bones I have found that human and ox bone contain as much as two per cent. of fluate of lime. It is therefore natural to suppose that the earthy phosphates dissolved in urine, which are chiefly derived from the decomposition and absorption of bone, should also retain the same proportion of fluate of lime. To prove it, I precipitated a large quantity of urine with caustic ammonia, collected and calcined the precipitate, mixed an ounce of it with as much sulphuric acid, and then heated the mixture moderately in a platina crucible covered with a glass plate prepared for etching. After some hours I removed the glass, took off the graver's wax, and found the lines corroded by the fluoric acid vapour.

Urine saturated by ammonia, filtered, and mixed with muriate of lime gives also a good deal of phosphate of lime, containing no fluoric acid. The urine therefore contains no other fluate than that of lime.

The *sulphuric* acid is also found in urine. The alkaline fluids merely neutralized by acetic or muriatic acid, and then mixed with muriate of barytes, give no indication of sulphuric acid. But if the residue left after the evaporation of these fluids, be first calcined, and the saline part then extracted by lixiviating the ash, and treated with muriatic acid and muriate of barytes, a notable quantity of sulphuric acid is found, produced from the sulphur contained in animal matter. But with urine the appearances are very different. Muriate of barytes added to it forms immediately a copious precipitate of sulphate of barytes; and I have constantly found that the quantity of sulphuric acid in urine exceeds that of the phosphoric acid. Rouelle the elder long ago detected sulphuric acid in urine, but this was considered as an accidental circumstance: I have however very good reason for supposing this acid to be a most essential constituent part of urine. The production of this acid takes place in the kidneys, and the action of these organs resembles combustion in this respect, that part of the constituent elements of the urine, such as the sulphur, phosphorus, the radicals of the alkalies and earths, &c. become oxidated to their *maximum*: and here also the kid-

neys generate some acids with compound bases. The remainder of the blood gives rise to the urea, which being formed from the materials of the blood, ought to contain more azote in proportion as a greater number of the other elements of these materials has been acidified. It would however be incorrect to consider the production of urine as an excretion of the azote which is in excess in the animal economy, because it appears that the amount of its quantity in the constituents of urine is not greater than in the constituents of blood. We might with as much reason consider the kidney as an organ for oxidation; but it is certainly an error to believe that any organ except the lungs has the office of removing one particular element in a greater proportion than the rest.

I at first conceived that the whole of the sulphur contained in the blood was acidified in the kidneys, and consequently imagined that the same change might be induced on a part of the azote, the carbon and the hydrogen. Proust asserts that the urine contains carbonic acid; but it is very difficult to establish the certainty of this fact, because the urea is decomposed by a heat even inferior to that of boiling water, and produces carbonate of ammonia, which is decomposed by the free acid of the urine, and carbonic acid is disengaged. I am rather disposed to believe that urine does not contain carbonic acid, because no bubbles of air are ever seen to form on the sides of the containing vessels: and

if urine still warm be thrown on any substance whatever reduced to powder, such as powdered sugar, no effervescence ensues. Urine is found to contain no nitric acid. If the residuum from evaporation be treated with alcohol, and the undissolved portion, (which must contain all the nitrates of the urine) be exposed to fire, it will exhibit no sign of detonation: although this phenomenon is very perceptible whenever a very small quantity of nitrate of potass or even of nitric acid has been added to the urine. Lastly, I found accidentally that the whole of the sulphur contained in urine is not acidified. I had made use of nitrate of barytes to precipitate the sulphuric acid in urine, and had previously added to the latter some nitric acid in order that the phosphate of barytes might be retained in solution. After having separated the sulphate of barytes, I precipitated the phosphate by ammonia, and after having filtered the ammoniacal liquor, I evaporated it. During its evaporation it deposited small white shining crystals, exceedingly hard, insoluble in water, in acids, or in caustic potass. After subjecting them to many experiments, I at length found them to be sulphate of barytes. In repeating the experiment with muriate of barytes and muriatic acid, in order to prevent the precipitation of the phosphate of barytes, no sulphate of barytes was formed. The production of this salt is the more singular because the nitric acid present was even supersaturated with ammonia. Instruct-

ed by this experiment, I precipitated another portion of urine with nitrate of barytes, and after filtering evaporated it to dryness, and burned the residuum with a fresh quantity of nitrate of barytes. The ashes treated with muriatic acid left a considerable quantity of undissolved sulphate of barytes.

4. *The benzoic acid* is found, according to Scheele, in the urine of infants. I have not been able, in my experiments, to discover the least trace of it, and I much doubt whether it be contained in acidulous urine.

5. *Lactic acid.* It is principally to this acid that urine owes its acid properties: and if I may be allowed to speculate on final causes, I should say that it is destined to hold the earthy phosphates in solution, and obviate the dire effects of their deposition in a solid mass. In order to ascertain the presence of this acid, the urine must be evaporated to the consistence of syrup, and treated with alcohol. The substance which remains undissolved is acid, and by the addition of ammonia is decomposed; and the lactic acid combined with the ammonia becomes soluble in alcohol. From its solution in alcohol the ammonia is disengaged by quicklime, and from the new salt thus formed the lime may be separated by oxalic acid, which leaves the lactic acid dissolved in water. By this process a small part only of the lactic acid contained in the

urine is obtained, the greater part, together with the lactate of ammonia, being carried off by the alcohol.

B. *The deposit from urine.*

Urine, by cooling, affords a deposit which varies considerably in different circumstances, not only in quantity, but also in external characters. When it is abundant the urine becomes turbid throughout, a grey powder is precipitated, and after continuing for some time at rest, the precipitate is found at the bottom covered with a mucous stratum. The deposit gradually acquires a red tinge, and after some time is found perfectly crystallized. When the urine does not become turbid, there only appears a thin cloud scarcely discernible, which by rest sinks to the bottom or collects in light transparent flocculi, in which there are sometimes formed, after 24 hours, red crystals.

All urine, when newly evacuated, contains a matter suspended in it, which in some degree affects its perfect transparency. This matter is the mucus of the inner coat of the bladder. If the urine, while yet warm, be poured on a filter, it will pass perfectly clear, and the mucus will remain on the filter in the form of transparent and colourless flocculi. The deposit which afterwards takes place in filtered urine is pulverulent, and nowise mucous: which proves that it is from admixture with the mucus of the bladder, that the flocculent appearance,

so frequently assumed by the deposit, arises. By drying the mucus loses its transparency, becomes red, and sometimes has a crystalline aspect, which is owing to the presence of uric acid, the crystals of which have a diameter exceeding the thickness of the dried mucus.

If a person after continuing for a long time, either in a standing or sitting posture, without much exercising his muscles, should evacuate urine successively in different vessels, the first portions will contain the largest quantity of mucus, the next less, and the succeeding portions none whatever. This arises from the mucus being heavier than the urine, and collecting in the lower part of the reservoir ; but if, on the other hand, a person be obliged to remain long on his back, and to evacuate the urine in that position, the lowermost portion does not come away first, and it then frequently happens that the whole of the mucus cannot be evacuated, and that a part remains for a long time in the bladder, when, from the disposition of the uric acid to form crystals in the mass of mucus*,

* I have very often observed that when the urine has been evacuated in two portions, of which the one contained mucus, and the other was clear, the latter has afforded no precipitate, while in the former the mucus was found after 12 or 24 hours, to contain numerous red crystals, sometimes of considerable size. It would therefore appear that in urine which does not contain enough of uric acid to form a precipitate by cooling, the presence of mucus favours in some particular way its crystallization.

the latter becomes a nucleus for the formation of calculus. To this circumstance may be ascribed the frequency of this disorder originating during the cure of fractures of the lower extremities.

The cloud which appears in the urine during fever, is in fact merely the mucus of the bladder which, from the increase of the specific gravity of the urine, subsides more slowly, or sometimes remains suspended in the fluid. The deposition which urine exhibits in diseases, frequently suggests useful indications, and its examination is sometimes of great importance in the practice of medicine. It is necessary, in this inquiry, to distinguish between two different kinds of matter deposited: the one being composed of the materials which are not held in solution, and are mechanically suspended, and the other consisting of those which are dissolved in warm urine, but which separate by cooling. The former contains, in a state of health, only the mucus of the inner coat of the bladder; which, while yet warm, may be separated from the latter by filtration. The mucus that remains forms discrete flocculi, which do not collect together, and which, after being dried, do not recover their original transparency and mucosity by maceration in water. This mucus is in great part dissolved by acetic acid or diluted muriatic acid; but diluted sulphuric acid acts upon it very feebly. It is also soluble by digestion in caustic alkalies.

In catarrhus vesicæ the urine is loaded with an enormous quantity of a mucous matter which is suspended in it. This matter is a true mucus, although, in consequence of the morbid affection of the organ which produces it, its characters are different from those of healthy mucus. If it be collected on the filter, in proportion as the water is absorbed, it becomes more and more mucous and viscous; and, during the desiccation, it becomes transparent and greenish. By maceration in water it recovers its mucous character, undergoes after some time an acid fermentation, and acquires a purulent appearance. In a word, the mucus of the bladder, when diseased, approaches more nearly to that of the nose, and differs much in its properties from the secretion in its natural state.

There is still another morbid condition of the urinary passages, in which the urine carries along a matter mechanically suspended in it, and which has so close a resemblance to that produced by catarrh, that inattentive practitioners easily confound the one with the other. The urine when filtered leaves a mucous matter on the filter, which however does not become transparent by desiccation, but gives, on the contrary, a white powder appearing only to the touch. This powder consists of phosphate of lime, and the ammoniaco-magnesian phosphate, mixed with the mucus of the bladder. The urine in this disease has lost all its free acid, it does not affect the colour of litmus paper, and I have-

sometimes even seen it restore the blue colour of litmus when it had been reddened by vinegar. In observing this reaction, the paper should be inspected immediately; if it be allowed to dry, it reddens from the decomposition of the ammoniacal salts, and this happens even when the paper is immersed in a solution of neutral muriate of ammonia, or even when the ammonia is in excess. The urine in this disease has also the property of yielding a precipitate by common muriate of mercury, in the same way as during a paroxysm of fever: a circumstance which is owing to the absence of the free acid*.

The secondary precipitate which is formed in urine, which has been filtered while still warm, is pulverulent, and contains, as is already known, the uric acid in abundance. In the first moments after its formation it is of a greyish white, but it gradually acquires a reddish hue, and the pulverulent

* I once had occasion to treat a man attacked with this disease. I endeavoured by large doses of phosphoric acid to supply the deficient acid in the urine, but without being able to effect any alteration whatever. The dose was at length increased till it purged the patient, when the urine suddenly resumed its natural characters, and became acid, transparent, and deposited uric acid. But these salutary effects disappeared with the purgative one, and they could not be again reproduced. After the phosphoric acid had been employed in vain, the acetic and sulphuric acids were given, but without success. Alkaline remedies had no effect either beneficial or injurious, and the disease still continues, having produced a great degree of debility in the inferior extremities.

matter assumes at the same time a crystalline form. The change is effected still more quickly when exposed to the immediate contact of air, than when the deposit is covered with urine. The greyish precipitate which is first formed is soluble in caustic alkali, without the evolution of ammonia; but in proportion as it becomes red and crystallized, potass disengages from it ammonia in abundance. It is evident, therefore, that the crystallization of the precipitate depends on the formation of urate of ammonia with excess of acid, which appears to constitute the red crystals which form in urine by cooling. I think I have observed that the mucus is in a great measure concerned in this change in the precipitate, since it takes place more slowly, and in a less degree, in urine which has been filtered. That species of urine which on cooling becomes milky and appears like a mixture of clay and water, yields about one thousandth of its weight of precipitate.

The deposit, treated with acetic acid, is partly dissolved and gives a yellowish solution, from which a precipitate is thrown down by carbonate or prussiate of potass, as well as by infusion of galls, but not by the caustic alkalies. The quantity dissolved is greater, and the yellow colour more intense when the urine has not been filtered before cooling; which appears to prove that the substance dissolved by the acetic acid is in both cases mucus, of which a part has been dissolved in the urine, and since precipitated in a state of chemical combination with

the uric acid. It is this compound which is gradually decomposed, and gives rise to the crystallized superurate of ammonia. The deposit contains none of the earthy phosphates. The muriatic acid digested with the deposit, and then saturated with ammonia, precipitates nothing. Subjected to fire, the deposit burns, and leaves at length, and with some difficulty, a very small quantity of a fused ash, which consists of carbonate of soda, proving that the deposit often contains a small quantity of superurate of soda, that insoluble salt which, according to the experiments of Dr. Wollaston, produces the gouty concretions of the joints.

The secondary deposit of healthy urine, therefore, is not uric acid, but a combination of this acid with an animal matter, which appears to be a portion of the mucus of the bladder dissolved by the warm urine. The deposit contains still a trace of superurate of soda, and by spontaneous decomposition there is formed superurate of ammonia which renders it crystalline.

It is to be presumed that uric acid, which is deposited in the bladder and forms calculi, contains this same animal matter, which ought accordingly to be an essential constituent of all calculi formed in the bladder. I have thus found it in two different calculi which I examined with this view. The following is the mode of separating the uric acid from the animal matter. The calculus is dissolved

in caustic alkali, and a precipitate obtained by adding to the solution muriatic acid in excess. The precipitate consists of uric acid; and a combination of the animal matter with muriatic acid, which may be carried off by washing the precipitate freely on the filter. The muriatic compound is soluble in pure water, and is again precipitated by the addition of muriatic acid, or by allowing it to drop into the acid liquor which has passed through the filter. The uric acid remaining on the filter is in a state of purity greater than any that has been hitherto the subject of examination.

C. *Analysis of Urine.*

I have been at much pains to arrive at as accurate a knowledge as possible of the precise composition of urine, both as to the quantity and condition of its constituents. The task has been laborious, difficult, and often extremely complicated. It would be trespassing on the patience of the Society, were I to attempt at present to give an account of all the details, and such is the nature of an exact analytical investigation that it admits not of a brief recital. I shall therefore content myself with communicating the general result which is as follows:

1000 parts of urine are composed of

Water - - - - - -	933.00
Urea - - - - -	30.10
Sulphate of potass - - -	3.71

Sulphate of soda	3.16
Phosphate of soda	2.94
Muriate of soda	4.45
Phosphate of ammonia	1.65
Muriate of ammonia	1.50
Free lactic acid, Lactate of ammonia, Animal matter soluble in alcohol, and usually accompanying the lactates, Animal matter insoluble in alcohol, Urea not separable from the preceding	17.14
Earthy phosphates with a trace of fluate of lime	1.00
Uric acid	1.00
Mucus of the bladder	0.32
Silex	0.03
	1000.00

With regard to the relative proportions of these ingredients, it is very probable they may vary independently of disease. I believe, however, that in urine they are never very different, unless from pathological causes which materially affect the health.

I should also observe that in the 17.14 parts of lactic acid, lactate of ammonia, &c. there exists a quantity of water which it is not possible to ab-

stract without the risk of decomposing these substances. The quantity of uric acid always varies according to the individual, and also in the same individual from different circumstances which have little influence on health. In the above analysis it was determined from urine which became turbid throughout during its cooling, and which during its deposition resembled water mixed with clay.

The earthy phosphates contain much more magnesia (as much as 11 per cent.) than in the bones or the ashes of blood. Of the cause of this I am ignorant; but I have likewise found much more potass in urine and in milk than in the blood.

The silex was not detected by the combustion of dried urine: for in this way it might have been considered as a constituent of some animal matter dissolved in the urine. I discovered it by treating evaporated urine with alcohol, then with water, and afterwards with muriatic acid, which left the undissolved silex in the form of a grey powder, which fused with soda produced a transparent glass, and which by the decomposition of the glass was converted into the gelatinous state. The water we drink, and which supplies the continual waste of that element by perspiration and by urine, always contains silex, which appears not to separate from it in the body, and which thus makes its exit in the same state in which it entered. It is evident that this earth should be found dissolved in the

other animal fluids, and that the quantity must vary according to the quantity contained in the water used as beverage.

Milk.

My experiments have chiefly been made on the milk of the cow. The composition of this fluid is exceedingly analogous to that of blood. It consists, like the blood, of a chemical solution, and an admixture of undissolved matter suspended in it. By exposing milk for some days in a shallow vessel to the temperature of 32° Fahrt. I separated from it the cream as completely as I could. The lower portion of milk, decanted by a hole at the bottom of the vessel, had a specific gravity of 1.033 and yielded by analysis the following constituents:

Water	928.75
Cheese, with a trace of butter	28.00
Sugar of milk	35.00
Muriate of potash	1.70
Phosphate of potash	0.25
Lactic acid, acetate of potash, with a trace of lactate of iron	6.00
Earthy phosphates	0.30
	1000.00

Cream contains the emulsive matter which is not dissolved, more concentrated and mixed with a portion of milk. This emulsion is easily decom-

posed by agitation, absorbs oxigen, and the butter separates: the milk becoming by this operation more acid than it was at first. I found that cream of the specific gravity 1.0244 was composed of

Butter	4.5
Cheese	3.5
Whey	92.0

As 92 parts whey contain 4.4 sugar of milk and salts, it follows that cream contains about 12.5 per cent. of solid matter.

It is very remarkable that scarcely any other alkali than potash is found in milk. I have burnt a quantity of dried milk, and have dissolved the muriate of the ashes in spirit of wine, and the alkali left undissolved by the alcohol, neutralized by the sulphuric acid, produced only sulphate of potash. I know not how far this observation is applicable to other kinds of milk, or to milk taken from other individuals.

Cheese, which is destined to be part of the nourishment of the young animal, has very peculiar characters, which, as it would seem, fit it for this office. It admits easily of incineration, affording a white ash which contains no alkali, and which forms as much as 6.5 per cent. of the weight of the cheese. This ash contains principally earthy phosphates with a little pure lime: but it contains nei-

ther alkali nor oxide of iron. Cheese digested with concentrated muriatic acid yields the greater part of its phosphates to the acid, and it afterwards burns without leaving any ash. But the cheese may be precipitated from the milk by an acid without losing its phosphates. It appears then, that the latter are not yet formed, but that a slight affinity only is requisite to their production. We may conclude that nature has thus sought to assist the digestive powers of the young during a period of their lives, in which there exists in the œconomy the greatest demand for earthy phosphates for the purposes of ossification, which is at that time advancing so rapidly.

Cheese is generally considered as a substance insoluble in water, and yet a great part of it is in actual solution in milk. A solution of it in water may be obtained, if cheese precipitated by an acid and well expressed, be digested with carbonate of barytes or carbonate of lime. The carbonate is decomposed with effervescence, and the cheese quitting the acid is dissolved. The solution is yellowish and resembles a solution of gum. Evaporated to dryness it leaves a yellow mass which easily redissolves in water. The solution boiled in an open vessel, becomes covered with a white pellicle, precisely as milk does, and acquires the smell of boiled milk. The membrane is almost insoluble in water, and appears to be a product by the action of air on the dissolved cheese.

With the mineral acids cheese produces the same combinations as albumen and fibrin, although the neutral combinations are less soluble than those of fibrin. A great excess of acetic acid is required in order to dissolve the cheese, and the neutral combination with this acid appears to be insoluble. Cheese is easily dissolved in alkalies. Its solution in acetic acid as well as in ammonia, becomes covered with a small quantity of cream every time that the cheese has not been well separated from the butter. Alcohol converts cheese into an adipocirous and fœtid substance.

Butter and sugar of milk are so well known, that no additional information can result from my experiments on them.

A CASE

OF

FUNGUS HÆMATODES.

By GEORGE LANGSTAFF, Esq.
SURGEON, NEW BASINGHALL STREET.

COMMUNICATED BY

WILLIAM LAWRENCE, Esq.

Read Nov. 10, 1812.

JOHN WHITEHORN, a boot-maker, about 30 years of age, of middle stature, rather corpulent, and of that sallow complexion peculiar to a female whose constitution has been much impaired by long obstruction of the catamenia; applied to me on the 24th of October, 1811, respecting a small tumor on his left shoulder, situated just below the spine of the scapula.

It was about the size of a cherry, and had a blueish red appearance.

In the situation of the swelling, there had been from the time of birth, a large mole, which, eighteen

months ago, had gradually fretted into a sore. From the latter a painful and occasionally bleeding fungus sprung up. Previously to this time, his general health had been exceedingly good.

Two other tumors, each about the size of a small nutmeg, had formed in the left axilla about a fortnight before I saw him. They were free from pain; and the skin covering them had its natural appearance.

I cut away the tumor from the shoulder on the 26th of October. The operation was attended with considerable bleeding from numerous small vessels. The removed part consisted of a firm purple vascular mass, originating from the cutis.

A disposition to the formation of fungus retarded the healing of the wound for five weeks; and the cicatrix had not a natural appearance.

The swellings in the axilla gradually increased, and united into one mass, which grew constantly larger, in spite of the employment of various local means, such as leeches, cold washes, and different stimulating applications. The health became affected, and the functions of the bowels particularly disturbed: small doses of mercury, purgatives, and tonics were employed; and a low diet enjoined.

By December the swelling had increased so much

as to occupy the whole axilla, from the pectoralis major to the latissimus dorsi; and it seemed to extend outwards, between those muscles, so as not to press on the axillary nerves and vessels. Its circumference and base were as hard as a cancerous breast: the body of the tumor was elastic, and seemed divided into sacculi containing fluid: the surface was irregular, and the prominences gave a feeling as of fluid contained in cysts. The skin was much stretched and discoloured: it had a purpleish hue, not unlike that of erysipelas; but its temperature was natural. There was no pain until the end of November, when a violent attack took place, with considerable fever: these subsided, leaving the patient occasionally subject to darting pain in the swelling.

One of the prominences at the upper part of the tumor enlarged considerably, and the integuments became thin. It evidently contained fluid, and was very painful when touched; but there had been neither throbbing nor heat in the part. It continued in this way for some time, causing great suffering and disturbance in the constitution. I determined to make a small puncture, to give the patient a chance of relief, and gain some knowledge of the nature of the tumor.

Four ounces of dark coloured blood flowed from the opening; and the discharge was followed by immediate relief.

I closed the aperture, which was much smaller than that usually made in phlebotomy, with adhesive plaster; but a considerable quantity of sanies and ichor flowed through it for nearly a fortnight, greatly debilitating the patient.

The tumor increased but little after this period, nor was pain complained of till the 12th of January, when excruciating and darting sensations were felt in the swelling, on the side of the neck, and along the under part of the arm. These were sometimes so severe, that the patient supposed he should not survive them. The circumference of the tumor now became considerably enlarged, the prominent parts continued elastic, and the integuments were of a deep purpleish red colour. The continued pain and constitutional derangement, accompanied with difficulty of breathing, confined him to bed from the 19th of January, and were not alleviated by any local or general treatment.

On the 8th of February, the tumor measured six inches and a half in the long axis, and six in the short; the central point began to project into a nipple-like eminence, which was apparently only covered with cuticle. It was surrounded by a beautiful deep red and purple coloured disk, the diameter of which was two inches and a half; the skin from this part to the extent of the swelling, was gradually shaded with purple and red streaks. It bore considerable resemblance on the whole to a

large inflamed female breast. To the violent pain already described, darting sensations were added, passing from the axilla through the chest, and accompanied with dyspnœa. During the paroxysms of pain, the pulse was from 100 to 120 in a minute, and on its abating sometimes as low as 69.

On the 20th the integuments of the nipple-like projection assumed a dirtiesh white appearance, at the most prominent point, and an ichorous discharge exuded.

The cuticle separated on the 22d, and exposed an opening about large enough to admit the end of a probe, through which a considerable quantity of blood and ichor oozed. Instead of a fungous growth, there was only a granular state of the surrounding cutis. The tumor still increased, and measured on the 25th nine inches by seven.

Feb. 27.—The discharge was still copious and perfectly inodorous; which indeed had been the case from its commencement. The integuments, at the centre of the tumor, were of a brownish red appearance, and so very thin to a considerable extent, that I was inclined to think they would give way.

Feb. 29.—The discharge was not lessened, the edges of the ulcerated part were beginning to ci-

catrize, but the aperture shewed no disposition to heal. I introduced through it the whole length of a common sized probe; it took an oblique direction towards the axilla. The discharge was not increased when the probe was withdrawn; although it might have been supposed from the fluctuation, that a large quantity of fluid was contained in the centre of the tumor.

Another round, hard and painful tumor, about the size of a pea, was discovered on the shoulder, half an inch from the one last mentioned.

March 1.—The swelling still increased in the horizontal direction, though there had been a considerable discharge of blood with serum; the integuments were of a brownish red colour, and felt extremely thin. A fourth tumor, similar to those on the shoulder, presented itself about an inch above the upper absorbent inguinal glands. His health now declined rapidly, he was much emaciated, and complained of constant and violent pain in his loins and chest, and his respiration was hurried on the following day.

March 2.—The discharge from the small aperture had greatly diminished; the tumor was much increased, all the patient's sufferings aggravated.

March 6.—From the last date to the present,

the lateral increase of the swelling had been astonishing, as its transverse measurement was now fourteen inches. The skin, about two inches above the part which burst and afterwards cicatrized (except the small aperture through which the fluid had oozed more than a fortnight,) was extremely thin, prominent, and likely soon to give way.

The circle, where the integuments first ulcerated, was considerably enlarged, owing to its distension by fluid, the aperture having been for some days closed; the new formed skin was changed into a white and fibrous substance, which adhered with great firmness. The pain in the tumor was at this time so particularly violent, from the integuments being so much distended, that I felt inclined to make a small puncture to afford him temporary relief; but on the evening of this day, the integuments gave way, and a considerable quantity of bloody serum was discharged, which occasioned immediate ease; but fluid still seemed contained in several parts of the tumor.

March 10.—The difficulty of breathing increased, and he had coughed up blood. The integuments at the most convex part of the tumor were beginning to ulcerate, and there were several apertures, through which a probe might be passed into it. The discharge was still considerable, though it did not tend to the diminution of the swelling.

The pain, difficulty of breathing, and other symptoms grew more and more distressing, and diarrhœa came on, with excruciating pain in the bowels. The tumor assumed an appearance of sphacelation at one part, and the discharge became very offensive.

Although the powers of the constitution were reduced to the lowest ebb, a disposition to form these tumors still remained, as one made its appearance on the right side of the neck, and another on the back, and the former became considerably enlarged. Violent sickness accompanied with tormina and tenesmus, and other dysenteric symptoms continued till the 16th of March, when death took place.

After insulating the integuments from the base of the tumor, and dissecting back those covering the chest and posterior part of the arm, the superficies of the diseased mass was found to be formed of a congeries of various-sized dark purple-coloured tumors, which had extended between the *pectoralis major* and *minor* muscles, as far as the origins of the latter and backwards two inches beneath the *latissimus dorsi*, but had not produced any alteration in their structure.

The axillary absorbent glands were larger than natural; and although the tumor adhered to the

capsular ligament, the consequence was only slight thickening of that part.

The vessels and nerves of the axilla were closely imbedded in the upper part of the disease, but except the musculo-cutaneus, or perforans Casserii, which was completely impacted, and lost in the tumor, they were not compressed to such a degree as to impede their functions, or produce any morbid appearance.

The morbid growth weighed four pounds averdupois weight, and when the integuments and cellular substance, covering the various-sized tumors which formed the whole, were removed, and the axillary nerves and blood vessels cleaned, it bore much resemblance to an enormous bunch of black Muscadine grapes.

All the tumors situated on the external part of the large one, had that spongy elastic feel peculiar to it, which so singularly characterizes this horrid disease, and which has been described by those who have written on fungus hæmatodes, but particularly by Mr. Burns, Mr. Hey, and Mr. Wardrop.

Several of the tumors were cut through, to examine their internal structure and contents; some were found to be composed of a soft medullary substance, like brain mixed with coagulated blood;

others with a dark grey-coloured substance in consistence similar to the former; a few were of a blackish colour, but had the same arrangement of structure as the former, and the fluid expressed from them was exactly like the pigmentum nigrum on the choroid membrane of the eye, or the inky fluid in the bronchial glands, and when their contents were pressed out, the condensed cellular cysts only remained, with a loose fibrous reticulated arrangement of their internal parts, with the ramifications of several minute blood vessels.

The main bulk of the disease was next divided; it presented an irregular union or blended appearance, similar to what composed the other tumors, and the contents were confined by strata of apparently fibrous reticulated condensed cellular substance; but there were three cavities, each of which contained about an ounce of loose coagulated blood.

The sloughing process on the anterior part of the tumor had not penetrated beyond the integuments, but had changed that part of it beneath them into a white sloughy looking substance.

The blood vessels were numerous, but very small.

There was a tumor in the sternum, under the periosteum, about the size of a walnut, of a brown-

ish red colour, and a pulpy structure. It had caused the absorbents to remove the bone, to allow of its projecting inwards and outwards, and was only held by the ligamentous membrane which covers both sides of that bone; the sternum was softened, and its cancelli filled with the same kind of matter through one half of it; several of the ribs on both sides had similar tumors in them, not far from their cartilages, and under the periosteum. The liver was of a paleish red colour, rather soft in texture, and bestudded throughout its substance with various sized tumors, contained by capsules; some of them consisting of medullary matter mixed with blood, others possessing exactly the same consistence, but of a cineritious colour, and intersected with cellular septa.

There was a small tumor similar to those in the liver, between the layers of peritoneum which form the ligamentum suspensorium hepatis; and two on the front of the pancreas, one about the magnitude of a pigeon's egg, the other the size of a hazel nut, but connected only by cellular substance to that viscus, which was perfectly healthy.

The stomach, duodenum, and jejunum, presented a healthy appearance, but the mucous coat of the ilium was considerably thickened, and seemed covered with a layer of coagulable lymph of a greenish colour: and I was led to suppose this idea correct, as the valvulæ conniventes were firmly

agglutinated, and their extremities thickly coated with the same substance.

The cœcum with its appendage and a great part of the colon, exhibited the same diseased appearance, only in a greater degree; and in some parts of those intestines there was an increased state of vascularity.

The iliac and lumbar absorbent glands were enlarged, but did not partake of the primary disease, as in the cases of medullary sarcoma, related by Mr. Abernethy.

The pericardium and heart were healthy; the lungs were studded in their substance with small tumors, similar to those in the liver, and there were many immediately beneath the pleura pulmonalis; and one of the lobes on the left side was loaded with blood and mucus.

On reflecting the integuments of the cranium, another tumor was perceived on its vertex, beneath the pericranium, which had never been noticed during the patient's life; it had affected the bone only in a slight degree.

On removing the skull-cap, a similar tumor was found on the dura mater, under the occipital bone; it had caused absorption of the surface of the bone, so as to expose the diploe. No deviation

from the natural appearances was noticed in the brain, except that the ventricles continued rather more fluid than usual.

The testicles were perfectly healthy.

HISTORY

OF A

SEVERE AFFECTION

OF THE

ORGANS OF RESPIRATION,

WITH THE

APPEARANCES ON DISSECTION, AND REMARKS.

By A. P. WILSON PHILIP, M.D.
PHYSICIAN TO THE INFIRMARY AT WORCESTER.

COMMUNICATED BY

DR. BAILLIE.

Read Sept. 8, 1812.

THE subject of the following detail was a gentleman residing about ten miles from Worcester, sixty-three years of age. In the early part of his life, he had served in the American war, in which, before he was thirty, he lost his right leg. He was of a fair and florid countenance; about five feet ten inches high, and of a strong and manly figure, his chest being particularly capacious and well formed. He was a man of quick feelings, but of

great resolution and benevolence. His habits were temperate. The loss of his limb prevented his walking much, but he generally spent many hours in the day on horseback.

For sixteen or eighteen years, he had occasionally hawked up a little mucus, tinged with dark coloured blood, without cough, to which he had never been subject. He had had several attacks of rheumatic fever; and was so much troubled with indigestion, that for some years he had been obliged to regulate his diet with great care. He had also been several times affected with a peculiar kind of dyspnœa, which I shall presently have occasion to describe, and which was most violent after his first attack of acute rheumatism. The dyspnœa was never attended with cough, but always with more or less expectoration of mucus tinged with dark coloured blood.

In the early part of last winter, he was much troubled with rheumatic pains, which prevented his usual exercise on horseback; and at the same time the symptoms of indigestion increased, and the dyspnœa returned, which last uniformly came on early in the morning, and obliged him to sit up in bed. These symptoms soon yielded to the use of rust of iron, extract of gentian, and infusion of bark, with gentle aperients, chiefly magnesia; and for a week or ten days seemed wholly to have left him.

They again returned however, notwithstanding the continued use of the medicines, and the dyspnœa began to be more troublesome, and to harass him during the day. It was now attended with some frequency of pulse, but without any other symptom of fever. He said the dyspnœa was always least after dinner, and seemed relieved by eating animal food and drinking a few glasses of wine. He complained much of a sense of sinking and debility at the stomach, and stated that he could relieve the dyspnœa by pressing hard on the epigastric region, or by a fuller inspiration than usual. The pulse was never either intermitting or irregular at any period of the disease*.

The character of the dyspnœa was very peculiar. It consisted of about twenty full, deep, and quick inspirations, succeeded by perfectly free breathing, for perhaps a minute, when the paroxysm returned. After several such alterations, the dyspnœa, in the earlier periods of the complaint, often ceased for many hours. In these intervals, as well as in the short intervals between the paroxysms, the patient could lie in the horizontal position without bringing on the dyspnœa, till within a very few weeks of his death.

* Unless it be regarded as an exception to this, that in the latter part of his illness, it was slower during the paroxysms of dyspnœa, than in the intervals between the paroxysms.

Various antispasmodic and other medicines were now tried without effect.

Having exposed himself to a cold wind on the 22d of last March, (for he still continued to ride out occasionally,) he experienced a sudden increase of the dyspnœa, and the pulse was for the first time rather hard. Although there was no cough, I advised the application of leeches, and afterwards of a blister to the chest, which gave considerable relief. This, however, proved of short duration, the sense of debility and fits of dyspnœa evidently increasing, though unattended with cough or fever, and the pulse becoming more frequent.

On the 30th of March, Dr. Edw. Johnstone of Birmingham was requested to see him, with me. It was agreed that he should take stomachic draughts with a small quantity of the vitriolated iron, and occasionally an anodyne draught, with tincture of opium and camphorated mixture, which he had been using.

Within two days after this, he was attacked in the night with so alarming a train of symptoms, that Mr. Ricketts (the surgeon under whose care he had been from the commencement of his illness) hardly expected him to survive till morning. When I saw him early in the morning, I found the dyspnœa extreme, but still appearing in paroxysms as

above described, unattended with either cough or fever. The pulse was very feeble but regular, and appeared to be about 160, though it could not be accurately counted. Laxative medicines and the occasional repetition of the anodyne were advised. Next day, (April 3d) the symptoms having by no means abated, Dr. Johnstone was again requested to meet us.

We now considered the case as hopeless, but on account of the great frequency of the pulse, agreed to make a cautious trial of the foxglove, hoping that it might, by lessening the frequency of the pulse, produce some mitigation of the symptoms. This we added to the anodyne draught; and directed two grains of ipecacúanha and four or five of rhubarb to be given occasionally, for the purpose of exciting the bowels.

While the pulse and breathing continued in the state here described, the sense of anxiety was extreme, and the feelings of the patient such, that he said he would rather die twenty times, than pass one such night as the two last. He was now occasionally affected with a degree of delirium. It is one of the peculiarities of this extraordinary case, that even when the fits of dyspnœa were worst, the patient never had any swelling of the jugular veins, unusual beating of the carotid or temporal arteries, or other symptoms of impeded return of blood from the head.

Soon after the first dose of rhubarb and ipecacuanha was given him, he was seized with vomiting and violent straining, which, although there was but little brought up from the stomach, continued for a considerable time. I was not displeased at this effect, as I had been long in vain urging him to take an emetic. The change of symptoms which ensued was very singular. The pulse at once lost its great frequency and feebleness, and became a good steady pulse of from 96 to 100. All appearance of dyspnœa ceased, the patient conceived himself almost restored to health, and said that if he recovered, the emetic had saved him.

I remained with him several hours after this, during which he continued perfectly easy, and the pulse was strong and good. I left him about six o'clock in the morning, telling him, that, as he was so much better, I should not see him again till next day.

On my return next day, I found that soon after I was gone, the dyspnœa had recurred with great violence; but the pulse, instead of being feeble and extremely frequent, was now only about 110 or 120; and not only very strong, but, as Mr. Ricketts, and Mr. Jukes, surgeon at Stourport, (who had seen him on the preceding day for the first time,) informed me, decidedly hard. Under these circumstances, they very properly took eight or ten ounces of blood from his arm, and repeated the

blood-letting on the following morning, to about the same extent, by which the symptoms were greatly relieved. The first blood drawn, shewed a little of the buffy coat; but there was no appearance of it in any of the succeeding blood-lettings. As mid-day approached, the fits of dyspnœa again returned, but by no means to the same degree. They had now, for many days, shewn a periodic tendency, being evidently worst about one or two o'clock, both in the day and night.

We then gave the patient a scruple of ipecacuanha, which produced repeated vomiting with much straining, and another very complete remission of the symptoms. Towards midnight, however, the fits of dyspnœa again returned, still, as usual, with perfectly free breathing between them, and without cough or fever. The pulse was a little above 100, strong and good, and in some degree hard.

On the 6th of April, I wrote to Dr. Baillie, giving a full detail of this very unaccountable train of symptoms, stating that blood-letting and emetics were the only means which had latterly afforded much relief, and that the relief from emetics had been the most complete. I was not then aware, that, although more immediate, it was of shorter duration than that from blood-letting, and that, at an earlier period, the symptoms had been wholly removed for a little time by bark, gentian, and rust of iron. Dr. Baillie advised the repetition of the

emetic, and the use of such tonic medicines as at the same time possessed some antispasmodic power.

Before I received Dr. Baillie's answer, the patient refusing to submit to the repetition of the emetic, we again had had recourse to blood-letting; and as Dr. Baillie had not mentioned blood-letting, I wrote to him on April 10th mentioning what had happened since my last letter. In his answer, he observed that the symptoms could neither arise from inflammation nor water. He recommended the repetition of blood-letting in the more severe, of emetics in the slighter attacks, and the use of such tonic medicines as had formerly been found useful.

The patient would on no account again submit to the operation of an emetic, and such symptoms of debility soon began to shew themselves, as deterred us from a frequent repetition of the blood-letting. After the third blood-letting, (which was to the extent of about six ounces, and was performed on the morning after the last emetic) the relief was more permanent than on any former occasion. Within the succeeding ten days small quantities of blood were occasionally taken from the arm. At first the loss of two or three ounces gave relief; but at length little or no advantage was derived from bleeding to this extent: and as the leg and stump began to swell, and symptoms of water in the chest appeared, the patient being now unable at any time to lie in the horizontal position;

and the secretion by the kidneys, which had for some time been diminishing, being now very scanty, we were afraid to have recourse to more copious blood-letting.

About this time a medical friend of the patient, Mr. Charlton, surgeon to the late Duke of Gloucester, came on a visit to him. He recommended the combination of James's powder with opium; and from this the patient derived so much temporary advantage, that he continued to the last to take half a grain of opium, and two or three grains of James's powder, two or three times a day.

As the foxglove had been discontinued from the sudden aggravation of the disease, after only one or two doses had been taken, we thought it proper to make another trial of it. For the first week it appeared to be of considerable service; but as we continued to give the opiate, it was difficult to say how far the relief was to be ascribed to it. The foxglove certainly produced no diuretic effect, nor could we succeed by other diuretics in at all increasing the secretion of urine. After the first week of its employment, the symptoms again began to increase; and we at length laid it aside.

We now again made a cautious trial of the rust of iron, from which so much advantage had formerly been derived; but a single grain almost immediately occasioned an aggravation of the symptoms.

A few days after this, we attempted to relieve the dyspnœa by taking blood from the chest by cupping. This procured an easier night, but its good effects extended no further.

The strength now declined rapidly, and there was almost constantly more or less delirium; but without any increased beating of the carotid and temporal arteries, or fullness of the jugular veins. During the delirium, and when the fits of dyspnœa were extreme, I put my finger on the temporal artery, and felt its beats with some difficulty. It was considerably more languid than natural. The countenance latterly was for the most part shrunk and pale, with a red well defined spot on each cheek, inclining to purple. His other medical attendants occasionally observed the face flushed; but this must have arisen from temporary causes, as it never occurred to me to observe it.

The last time I saw him, (which was two days before his death) the pulse was about 110, perfectly regular, and by no means feeble. During the last week, to relieve the delirium, a blister was twice applied to the head, with some temporary advantage. A short time before death, the breathing in the intervals of the dyspnœa appeared to be wholly suspended, and he expired on the 21st of May without a struggle.

The body was examined about forty-eight hours

after death. On a general view of the abdominal viscera they appeared healthy, with the exception of a greater degree of redness and a more vascular appearance of the small intestines than is usual. Our time for the inspection of the body being rather limited, we proceeded immediately to the examination of the thoracic viscera, where it was evident that the immediate cause of the disease had existed.

On opening the thorax, about three quarts of water were found, chiefly occupying the right side. The right lobe of the lungs appeared diminished in size; but, not more than was to be expected from the quantity of water found in this side of the thorax. The whole of the lungs appeared of a darker colour than usual, the pulmonary vessels being in a state of great congestion with very dark coloured blood. Part of the right lobe, for the space of about an inch and half in diameter, was converted into a substance like liver, and at this place only, there was an adhesion of the pleura. A small part of the edge of this lobe was of a white colour. In all other respects the lungs were healthy.

The pericardium was found every where firmly adhering to the heart, and the heart itself, especially on the left side, appeared of a dark red colour. There was no other indication of disease in it. The semilunar and other valves appeared in the natural state.

Most of these morbid changes were apparently the consequences, not the cause, of the disease. That the fluid found in the chest is to be regarded as a consequence of the disease is obvious; for the patient, till within about a month of his death, could, even in the shortest intervals of the dyspnœa, lie in the horizontal position without any inconvenience. That the inflammatory affection of the heart must be regarded in the same point of view, appears from a variety of circumstances; and particularly from this, that, at an early period, the symptoms were wholly removed by the use of bark, iron, and gentian, and the patient always found himself easiest after eating animal food, and drinking wine.

It is almost unnecessary to observe, that, had the dyspnœa arisen from the inflammation of the heart obstructing the passage of the blood through the lungs, it was impossible that the patient could, every five minutes, have had an interval of perfectly free breathing.

The pericardium every where adhered closely to the heart, giving reason to suppose that the adhesion had been of late date*. It may appear at first view very surprising, that inflammation of the heart should have existed, for however short a time, without betraying itself by any of its diagnostic symptoms. There was no fever, permanent dyspnœa,

* See Dr. Baillie's Morbid Anatomy, page 6th.

or cough*; and the symptoms, which occasionally attend inflammation of this organ, were also absent; viz. irregular pulse, palpitation, and syncope. In the collections of Bonetus and Morgagni, the reader will however find cases related, which prove that there is a chronic inflammation of the heart, which, not impeding its functions, may exist without betraying itself by any symptom; and can only be detected by dissection.

We might at first view be inclined to suppose, that the heart is subject to this species of inflammation from its being an organ of very dull feeling notwithstanding its importance; for it has met with severe wounds†; worms and other extraneous bodies have lodged in it‡; and abscesses and ulcerations have formed in it§, frequently without the production of any painful sensation, and what is still more extraordinary, of any material derangement of its functions. But we find the same species of inflammation occasionally existing in organs of the greatest sensibility.

The reader will see instances in which the intestines were inflamed, with little or no sensation of pain, in the 35th epistle of Morgagni, in Van Swieten's Commentaries on the aphorisms of Boer-

* Dr. Cullen's Synopsis Nosol. Method. genus XIII.

† Medico-Chirurgical Transactions, Vol. II.

‡ Sepul. Anatom. of Bonetus. lib. 2. sect. 8. and 19. obs. 39.

§ Sepul. Anatom. lib. 2. sect. 10. Additamenta obs. 2.

haave, Sir John Pringle's Account of the diseases of the army, and in the fourth volume of De Haen's Ratio Medendi. The case before us, may be regarded as another instance of the same kind; evident marks of inflammation appearing on the small intestines, although the patient had never complained of pain in them, nor had their functions been any further deranged, than to require the use of small doses of magnesia. The same species of inflammation has been found in the stomach itself, of all our organs perhaps the most sensible. De Haen, in the work just alluded to, mentions one case, in which the stomach was found inflamed after death, where there had been no vomiting; and another in which there had been neither vomiting nor pain.

Such cases lead to a curious inference, that the derangement of function in an inflamed part, depends more on the irritation of its nerves, than the distension of its vessels: for it appears from these cases, that, if the latter exists without occasioning the former, the function of the part is often little, if at all, impaired. The nerves appear to perform a part in the production of inflammation, which is, and probably ever will be, but little understood. In extracts from a work by Pedratto, in the 21st epistle of Morgagni, and in the Historia Hepatica of Bianchus, the reader will find cases and dissections related, in which the cause that produced the inflammation, was wholly confined to

one organ, and the inflammation to another. The interesting nature of the subject will apologize for this digression.

It is difficult to judge of the kind and degree of inflammation by examining the inflamed part after death. In the case before us, the appearance of the heart seemed to all present but myself to indicate a state of active inflammation. To me it appeared too flaccid, and of too dark a hue to afford this inference; and its colour seemed to me in a great degree to depend on the same cause, which gave the dark colour to the lungs, the vessels carrying very dark coloured blood. It is well known that, in highly inflamed parts, the blood retains a very florid colour long after death*. That inflammation had existed to a considerable degree, there can be no doubt, from the general adhesion of the pericardium; but chronic inflammation of the heart, as appears from the dissections just alluded to, is capable of producing even abscess and ulceration.

We cannot conceive that any great degree of dyspnœa could be occasioned by the diseased portion of the lungs, which was comparatively so small. Besides we may say of this, as of the inflammation of the heart, that had the dyspnœa arisen from such a cause, it must have been permanent. We cannot tell whether this change of struc-

* Dr. Baillie's Morbid Anatomy, page 2. and other passages.

ture in the lungs had existed from the commencement of the disease, because it might have existed, without betraying itself by any sufficiently marked symptom. But I think we may very positively say, that it was not the cause of the disease.

On reviewing all that has been detailed concerning this extraordinary case, the following observations naturally arise. The explanation which they afford must be admitted to be in several respects conjectural; but I know of no other which can at all account, either for the symptoms, or the effect of the means employed.

It appears, from the whole course of the symptoms, that no permanent course of obstruction to the circulation through the heart or lungs existed; and, in the dissection, no such cause could be found: for, that the inflammation of the heart only supervened in the latter stage of the disease, and that, when it did supervene, (as in the cases mentioned by Bonetus and Morgagni) it occasioned no obstruction to the circulation, as I have already had occasion to observe, are proved by the history of the disease. It follows then, that, without organic derangement, spasm, or any other cause capable of obstructing for the space of a minute the circulation through the heart and lungs, (for the fits of dyspnœa never lasted so long as a minute) some cause existed capable of occasioning such fits; and consequently of impeding the oxygenation, or, we

ought rather to say, from the late very accurate experiments of Mr. Ellis*, the decarbonisation of the blood in the lungs; and that such a cause did exist to a greater or less extent, from the commencement of the disease, appears from the blood hawked up having always been of a dark colour. This state of the lungs being admitted, does it not afford a ready explanation of the peculiarities of the foregoing case? We know from direct experiment, that the left side of the heart cannot be excited by venous blood. This does not seem surprising, as arterious blood is its natural stimulus. And may we not safely infer, that the smaller ramifications of the pulmonary artery, and the trunk, as well as all the ramifications of the pulmonary vein, which, in the natural state, carry only arterious blood, are also insensible to the stimulus of venous blood, which is the natural stimulus of the right side of the heart, and of the trunk and larger branches of the pulmonary artery. As soon, therefore, as the decarbonisation of the blood began to be impaired, the action of the former set of vessels would be impaired in the same proportion. Would not the consequence of this be, that these vessels would require a greater degree than usual of the stimulus of distension to excite them to carry

* An inquiry into the changes induced on atmospheric air by the germination of seeds, the vegetation of plants, and the respiration of animals, by Daniel Ellis, 1807.

Farther inquiries into the changes induced on atmospheric air &c. by the same author, 1811.

no the same quantity of blood, and that they would become subject to a constant repetition of temporary congestion, i. e. of such preternatural distension as was necessary to excite them to vigorous action*, by which means their power could not fail to be gradually impaired.

Emetics would necessarily give immediate relief, by forcibly compressing the lungs, by which the congested vessels would be emulged, and at the same time excited to a more vigorous action. It was evidently the effort of straining which gave relief. Little was brought up, and what was brought up was of an inoffensive nature. In the same way, relief was obtained by pressing on the epigastric region.

Blood-letting would necessarily relieve, by lessening the quantity of blood thrown into the debilitated vessels. The effects of the blood-letting would prove more permanent, because they would continue till the increasing quantity of blood, or increasing debility of the vessels, again called for a diminution of the blood. The good effects of the emetic would be more transitory, but more immediately felt, because, by it, the distended vessels would be immediately enabled to contract; but, the quantity of blood to be circulated remaining

* The unusually distended state of the vessels of the lungs with dark coloured blood, was evidently a necessary consequence of the continuance of this state of them.

the same, they would in a short time again be oppressed. Now all this was exactly what we repeatedly witnessed. The circumstance of the patient obtaining immediate ease, in slight paroxysms, by a very full inspiration, seems to indicate, that nothing was wanting for the free circulation through the lungs, but a more perfect decarbonisation of the blood.

It may also be seen, why there was no impediment to the return of blood from the head. For as the right side of the heart and larger pulmonary arteries possessed their natural stimulus, it is evident, that, long before congestion could take place in them, from the cause I have mentioned, death would ensue.

It may also be observed, that the deficient decarbonisation of the blood could never, in this way, go so far, as greatly to affect the whole mass of the blood. In the case of a blue boy it does so; because there, arterious blood is, to the last, supplied to the left side of the heart and pulmonary system, although this may be in too small quantity. But here, the defect is first felt in these organs, and consequently cannot go to any great length without destroying their action.

If these observations be just, the foregoing case affords an instance of a diseased state of the lungs, which, as far as I know, has not been noticed by

writers. And indeed the symptoms were such as it seems difficult to suppose could have arisen from any of the common causes of pulmonary disease.

It is not unlikely that this diseased state of the lungs, whatever it was, had given rise to the change of structure observed in one part of them, and which, could the functions of life have gone on, would probably have spread to the rest.

AN ACCOUNT

OF A

NEW MODE OF TREATMENT

IN

CHRONIC RHEUMATISM,

AND ESPECIALLY IN SCIATICA.

COMMUNICATED

BY ALEXANDER MARCET, M.D. F. R. S.

ONE OF THE PHYSICIANS TO GUY'S HOSPITAL.

Read Nov. 24, 1812.

I HAVE frequently had the opportunity of observing, for the last six or seven years, that the profuse and unavailing sweats which often spontaneously take place in the early stages of rheumatism, and exhaust the strength of patients without alleviating their sufferings, are almost in every instance checked, and the pains proportionally relieved, by the use of antimonial medicines. Several distinct instances of this kind were recorded some years ago at Guy's Hospital in our clinical diaries, and the explanation which I ventured to offer of this paradoxical result, was, that the profuse flow of moisture from the pores, is not, in itself, the circum-

stance which diminishes pain in rheumatic affections; but that the relief is produced by a certain condition of the surface, or peculiar action of the cutaneous vessels, which, though generally productive of moisture, is not necessarily connected with profuse perspiration. It is this peculiar action which antimonials are so apt to promote; and there is no difficulty in conceiving, how the violent and colliquative paroxysms of sweating which occur in rheumatism, gradually yield to this gentle and uniform operation*.

The following interesting case, which I think in every respect worthy of being communicated to the Society, appears to me to contain ingenious and instructive hints on the treatment of rheumatism, and on various physiological points, and to throw some light upon the preceding observations. It was drawn up by the patient himself†, a gentleman, who, although not belonging to the medical profession, is so well known in the philosophical and literary world, that his name, had I been at liberty to give it at full length, would have added great weight to the opinions and statements which his paper contains.

* It is probable also that the invariable tendency of antimonials to determine gently to the intestines, may contribute to this beneficial effect. Opium alone, often checks the most distressing symptoms, but its mode of operation is altogether different.

† R. C. Esq. F. R. S. &c. &c.

"About eighteen years ago," says Mr. C. "I received a sprain on horseback which occasioned very violent pain in my loins, and still more in the left hip, thigh, and leg, following the course of the sciatic nerve, and spreading itself over the sole of the left foot. I was then on the continent at Bruxelles; and after severe blistering and other remedies, not finding myself better, I was ordered to try the mud baths and douches of St. Amand. Having used them six weeks to no effect, I proceeded to Paris. I tried various methods of cure under the most eminent surgeons there, as warm and cold bathing, vapor, air conducted through a heated pipe directly to the part affected, warm and strengthening plasters, fumigations, &c. till at length the celebrated and unfortunate Monsr. De Seaux proposed the moxa. This was accordingly applied with more than common severity, and the suppuration was maintained longer than usual, but without effect. In this state I was, with many other English, put into a damp prison, and a violent rheumatic affection fell upon the injured parts. I remained in this situation fifteen months without a possibility of applying any remedy. Upon recovering my liberty, an English physician proposed to me a quack medicine, known in Paris by the name of Goderneau's powders*,

* I have since that time had an opportunity of examining these powders chemically. They are a preparation of mercury. Each parcel given as a dose contains 12 grains (French). The colour is

adding strong testimonies in its favour, although the French physicians reprobated it as unfit for human creatures. Much more convinced by the English physician than deterred by the French practitioners, I began this powder; and, after six doses of it, the pain diminished, and I was able to walk. Time has since that helped to wear out the complaint, and I have occasionally been five or six years without its

is not so white as that of calomel. Examined with a lens, small globules of metallic mercury are discernible, and also some reddish particles which are red precipitate. They are wholly volatile at a low heat like calomel, but volatilization separates them in a manner which proves them not to be homogeneous, for the vial in which they are sublimed is marked with three distinct zones, white, red, and grey. Water does not dissolve any sensible portion of them. Nitric acid dissolves the whole of them, and nitrate of silver poured into the solution lets fall a quantity of muriate of silver corresponding to about nine grains of calomel. The remaining three seem composed of one and a half metallic mercury, and one and a half red precipitate. By triturating the above substances in the above quantities, I produced a compound very like Goderneau's powders, but rather more uniform in its appearance. This is a very rude preparation of mercury, and the use of it should not be encouraged. Its effect is to produce a disagreeable sensation in the stomach, and afterwards to increase the appetite. It also purges, and, as I am told, sometimes produces vomiting. The French faculty exclaim against it, but this they are apt to do against every medicine more active than orange-flower water; and yet they admit corrosive sublimate. The Chevalier Goderneau was a military man, and a knight of St. Louis, but no chemist; and since the revolution has swept him from off the face of the earth, the care of preparing the powders has devolved upon his sister, an old maiden lady, who from time to time swallows large doses of them for a sore foot, which has the advantage of always being about to heal.

returning, sometimes travelling in frosts and snows in the north of Europe, passing two severe winters in the mountains of Germany, and sometimes inhabiting very warm climates. About five years ago, on going from Spain to Paris at a bad season of the year, without having the precaution of adding to the clothing I had worn in the south, I felt a slight return of the complaint. A dozen baths and douches of artificial sulphureous waters, which are prepared in a particular establishment* in that city, relieved

* The establishment of artificial mineral waters at Paris is very extensive and useful. All the known mineral springs in Europe are there prepared according to the best analysis of modern chemistry; and in sufficient quantity for drinking, bathing, or douches. The climate of Paris is colder in winter, and warmer in summer, but it is much less damp than that of London; and during five months of the year a continuance of fine weather is much more to be depended upon. Almost every evening from the beginning of May to the middle of September, all the public walks which abound in and near that city are crowded with persons who, very lightly clad, remain walking or sitting in the open air, till eleven or twelve o'clock at night, a system of life which no inhabitant of England could follow eight days consecutively without suffering for it. Warm bathing is therefore not of such general use or benefit in England as in France. There are few places where nature has done so much in favor of warm bathers as Bath; and art has done almost every thing except taking advantage of those very waters which have caused the prosperity of all the rest. The baths however have been considerably improved of late years, but still there does not exist at Bath so efficient a douche as those of the artificial establishment in Paris. The strongest douche at Bath falls from a perpendicular height of fourteen feet, as I have been informed. The highest douche at Paris falls from 32 French feet, a height

me, and I was wholly free from pain till I came back to England in 1809.

"No changes from heat to cold ever injured me so much as damp; and I had not long been in this country when the pains recommenced. They were not very troublesome at first, and during eighteen months I applied no remedy but increased clothing. In May, 1811, I grew much worse, and in October the climate of Scotland, where I then was, so much increased my sufferings that I found it absolutely necessary to have recourse to more active methods. By the advice of Doctor Gregory I went through a course of calomel, taking from two to three grains of it with a certain quantity of opium every twenty-four hours, during six weeks; and seconding its efforts with frequent blistering. I have applied as many as three blisters at a time, from my hip to my foot, and renewed them as soon as the skin was sufficiently healed to allow a fresh

height which is wholly arbitrary as to the medical effect, and is merely founded upon this physical fact; that the weight of a column of water 32 feet high, is equal to the pressure of the entire atmosphere upon a basis equal to that of the column of water. But, as the force of falling water increases in the ratio of the square of the perpendicular height of the pipe through which it falls, the douche at Bath has not one fourth part of the force of that in Paris, and the difference of their effect is very great. A small expence might rectify this deficiency at Bath, and establish douches of any height from one to fifty feet if necessary; and which, in their application, might be proportioned to the sensibility of the patient.

application. By other medical advice since that period I have tried bathing in warm sea water, in artificial sulphureous waters, the same as those which had afforded me relief in Paris, and in the Bath waters. I have also used the dry pump there, vapor baths, and heated air, friction, cupping, leeches, and electricity. I have taken internally nitric acid, James's powders, guaiacum, nitre, cicuta, hyoscyamus and the eau medicinale; bark, and finally arsenic, but all to no purpose. Were I to state in general terms the effect of all these, I should say that cicuta, hyoscyamus and the eau medicinale procured me temporary relief; and that bathing of whatever kind usually made me worse. In short, I had nearly exhausted the materia medica, and I had no hope left but in a more favourable climate.

"Happening about five weeks ago to hear of a case which bore a strong analogy to my own, though the subject was not a human creature, it suggested to me an experiment which I resolved to try immediately. A celebrated race-horse*, belonging to a nobleman very well known at Newmarket, had been cured of a disorder which had all the symptoms of rheumatism, by sweating in body clothes, after every other remedy had failed. I therefore clothed myself in a sufficient quantity of flannel, and set out to walk as far and as fast as I could. With the utmost difficulty I proceeded half a mile; and the pain I suffered contributed not a little to the effect

* Vandyk.

of the exercise in promoting perspiration. I returned home in a profuse sweat, rubbed myself dry before a fire, and went to bed. In about an hour I got up, found myself very much fatigued, but in other respects not worse. Forty-eight hours after this, I repeated the same kind of exercise, and found that I could walk a mile with as much ease, as I had walked half that distance on the first day. My general sensations were the same as before; but, as the fatigue diminished, I thought I could perceive an amendment in my rheumatic pains. Two days afterwards I took my third walk, proceeding as before, and after it I had a better night, less interrupted by pain than any I had enjoyed for eighteen months. From that moment I looked forward with confidence to a cure, and I have not been disappointed. Every succeeding walk has diminished my sufferings, and I may safely say, that after the sixth, I was as free from pain as I had ever been in my life. The only remnant I have left to remind me I was so lately a cripple, is a weakness in the left leg, particularly about the ankle, together with now and then a slight sensation of numbness along the sciatic nerve. These are natural symptoms, considering that I had been so long without using the limb; and there is every reason to suppose they will yield to time and moderate exercise. I have now walked nine times in this manner for the purpose of sweating, and shall continue to do so probably much longer; for I find it of the greatest use to my general health to counteract the effects of sedentary habits.

"I usually proceed to my sweating walks in the following manner. Next to my skin I wear stockings, drawers, and a shirt, all of fleecy hosiery. Over these I put one, two or three pair of flannel drawers, one, two or three flannel waistcoats; and round my hips and loins I gird six yards of thick flannel, making beside the drawers and waistcoats eight thicknesses of flannel on the chief seat of pain, and the origin of the sciatic nerve. Over all this I wear warm pantaloons and a great coat. When I have walked one or two miles, more or less according to the heat of the day, I am generally in a profuse perspiration. I return home, take off my wet clothes, have a couple of changes of well aired flannel, and then lie down upon a bed not warmed. I use no means to excite further perspiration after the muscular action is over; but, on the contrary, rather incline to check it as speedily as I can, taking particular care however to avoid catching cold.

"I do not perceive that the quantity I perspire, has any influence on the efficacy of the remedy. I imagine that a violent action produced in the general system is the chief cause of its salutary effect. In consequence of this opinion, I cease the exercise the moment that a very increased action is well established. This is fully produced with the above quantity of clothing in moderately warm weather, by walking from one to two miles. For patients who are very much disabled, the quantity of clothing might be increased, and the distance diminished.

When the excitement is well established, I find my pulse rise to between 90 and 100, and it is full and strong.

"When I began to experience relief from this mode of treatment, I was eager to prosecute the cure, and took my walks every second day. Being now relieved, I resume them only every fourth, fifth, or sixth day, as most convenient.

"I do not find myself under the necessity of any particular precaution as to avoiding cold the day I exercise; and, in this respect, sweating by muscular action has an immense advantage over warm baths, vapour, or heated air; particularly in a climate so damp as that of an island situated in the north seas must be. Physiology will easily point out many other advantages in exciting perspiration by a stimulus which invigorates, rather than by the enervating methods of heated air or water.

"Since beginning this process, I do not find that I am grown thin; and in my general health I am infinitely better. I feel myself more strong and active, and less sensible of cold. My diet has been much as usual, but my appetite rather increased. I have occasionally taken a little bark to maintain the general tone of the system; and on returning home after each walk, I have found that a few drops of hartshorn in water were refreshing, and prevented thirst during the rest of the day.

"By what I have learnt from very good authority, the exercise I perform is not one-twentieth part of what the Newmarket riders undergo. There is hardly an example of one of those who follow the advice of a skilful physician in their process of wasting, that suffers by it; and the opinion that either their health is injured or their life abridged, is altogether erroneous. Excessive purging is never used now, except when the riders are too lazy to undergo violent exercise, for purging is found by experience to be a much more prejudicial mode of reducing their weight. Sweating by muscular action with an increased quantity of clothing might be recommended not only to rheumatic patients as a safe and easy remedy for their pains; but to persons of both sexes who are troubled with redundant corpulency.

"*P. S.* Since the above was written, I have had leisure to make some further observations which I beg leave to add.

"I am still more convinced that a very profuse and long continued perspiration, promoted by muscular action, is not necessary to the cure of rheumatism, or to the improvement of general health, unless corpulency be one of the evils which is to be removed. Perspiration may be useful as a proof that

a very strong action is established in the system, and may in some sort be regarded as the measure of that action; in the same manner as the point of ebullition is referred to as a rough thermometer by those who want very warm water. Of the very much which has been attributed to sensible and insensible perspiration a great deal certainly is true; but may not much also be attributed to the action excited in the system by those very medicines which are supposed to promote perspiration? Does mercury, independantly of the diseases it cures or causes, diminish the quantity of flesh by mere perspiration?'

"As an encouragement to those who might be deterred from the use of this remedy by the apprehension that it is too severe for a weak constitution, I add the following table of my weight taken twice or thrice each day, during the time that I was performing six of these sweating walks, (one in every forty-eight hours) undertaken with a view to ascertain whether the quantity of exercise necessary to cure a sciatica, in such a case as mine, is so great as to cause a material loss of flesh in the patient.

"Before I began to weigh I had already walked nine times. It is therefore only by my general appearance that I can say I had not lost flesh by my first walks. Previously to the six walks of which I am going to state the results, I had been obliged to suspend my exercise for fifteen days, as the weather

had been extremely bad, and I had an accidental attack of diarrhœa during four days. I weighed night and morning, before and after walking, always in the same clothes, and these as few as possible, and took every precaution against error.

	WEIGHT.						
	Morning.		After walking.		Night.		
Nov. 26	st. 13	lb. 1	st.	lb.	st. 13	lb. 1½	
27	13	1	12	13	13	1	Walked 40 minutes; sweat profuse.
28	13	0			13	2	Out in a carriage most of the day.
29	13	1	12	12½	13	2½	Walked near three miles; sweat very profuse.
30	13	1			13	2½	Out on foot all the morning.
Dec. 1	13	1	12	13	13	2	Walked 30 minutes; sweat moderate.
2	13	1			13	3	Out on foot.
3	13	2	13	0	13	3	Walked 30 minutes, with increased clothing; sweat profuse.
4	13	1½			13	3½	Out on foot most of the day.
5	13	2	13	0½	13	3	Walked 30 minutes; sweat moderate.
6	13	1½			13	3	At home all day.
7	13	2	13	0	13	3½	Walked more than three miles; sweat very profuse.
8	13	2			13	4	Out on foot all the morning.
9	13	2½			13	4	Out on foot.

APPENDIX

TO THE

PAPER ON

CYNANCHE LARYNGEA.

By J. R. FARRE, M.D.

Read Dec. 22, 1812.

WHEN the cases of Cynanche Laryngea, which I had the honor to communicate to the Society, were read, the learned President, Sir Henry Halford, noticed the deficiency of communication in the various medical records respecting this form of cynanche, and briefly referred to the cases of Dr. David Pitcairn, and Sir John Macnamara Hayes, as instances of the same species. Respecting the former only of these distinguished physicians, was I at that time successful in obtaining the particulars which I wanted. In an interesting biographical memoir of Dr. David Pitcairn, published in the Gentleman's Magazine for April, and in the European for June, 1809, I read the history of the disease which deprived the world of this excellent

physician. A more valuable report, giving an account of both cases, has been recently presented to the profession by Dr. Baillie, in the third volume of the Transactions of a Society for promoting Medical and Chirurgical Knowledge.

The conversation to which my communication gave rise, has induced me to offer some observations on the diagnosis, and treatment of a disease which seems to be little known, and which assumes a more than usual interest from its having proved fatal to two physicians, notwithstanding their own practical knowledge, and the aid of the most eminent in the profession.

In all the cases of cynanche laryngea, the mucous membrane investing the epiglottis, glottis, and chiefly the external and posterior surface of the larynx, was the seat of the inflammation, by which the rima glottidis was so much narrowed, that the vital functions were actually extinguished by the stricture. It is true that the tonsils, the arch of the soft palate, the uvula, the tongue, the internal membrane of the larynx and trachea, all, or several of them were inflamed; but this was only an extension of the action to one common membrane which invests the whole, whilst it was most intense at the mouth of the larynx. In both the cases communicated by Dr. Baillie, uneasy sensations were felt in the larynx. In one of them, this symptom appeared at the accession of the inflam-

mation, and strongly excited the patient's anxiety for the result, at a time when the apparent inflammation in the fauces was so inconsiderable, that upon a superficial observation it would hardly have been noticed. I did not see Essex alive; but the slight swelling of the uvula and tonsils which appeared on dissection, made it obvious that his disease could not have been mistaken for cynanche tonsillaris. In Case II, when I desired the patient to point out the seat of his suffering, he placed his fore-finger on the middle notch of the thyroid cartilage, to which the epiglottis is fixed. The difficulty of the diagnosis does not in fact lie between this inflammation and the cynanche tonsillaris, but between it and the cynanche trachealis. Dr. Pitcairn wrote on a piece of paper, that his complaint was croup. Croup very nearly approaches the cynanche laryngea, but the difference between the seats of the inflammation will appear by comparing the symptoms and morbid appearances which belong to each disease. In the cynanche laryngea the symptoms are, uneasy sensation in the larynx, difficult and painful deglutition, partial swelling in the fauces, a supervening and perpetually increasing difficulty of breathing, inflammatory fever. In the cynanche trachealis there is a difficulty of breathing*, without any swelling in the fauces, or painful deglutition, the expirations, especially in cough

* The difficulty in the respiration is the symptom by which the danger must be estimated, for the ringing cough, without that adjunct, is of little importance. Many confound merely this symptom.

ing, are very shrill, the fever is inflammatory. In both, the voice is changed, and, in extreme cases, is suppressed; the termination is by suffocation.—The following are the morbid appearances: in the former, the mucous membrane investing the epiglottis and margin of the glottis is inflamed; serum is effused under it, or coagulable lymph on its *external* surface, by which the *rima glottidis* is narrowed, or actually closed. (See plate III.) In the latter, the mucous membrane of the larynx and trachea is inflamed, and a layer of coagulated lymph is formed on its *internal* surface, from the extremity of the epiglottis to an indefinite extent within the trachea, by which the *tube* itself is narrowed, or actually closed. A puriform fluid, instead of mucus, is found in the trachea and bronchia. These characters chiefly apply to simple cases of both species, for as they differ in their seats, but not in their nature, it is possible for both forms to be combined in the same patient, and it were an easy matter to prove that a still greater extension of inflammation than this does actually take place in these organs.

In comparing the morbid appearances with the symptoms of cynanche laryngea, it is not difficult to determine that the inflammation, from its limited

symptom, which also not unfrequently occurs in the catarrhal affections of children, especially during dentition, with croup, which is comparatively a rare disease.

seat, from its not exceeding the bounds of the adhesive stage, and from the symptomatic fever, requires the use of the lancet. I may be here allowed to give an unbiassed opinion; for in neither of the cases which I have described did I order general bleeding: the first, I have already stated, was not attended by myself; the second was not in a state to admit of this remedy when I was consulted. Both were bled, it is true, but it was done in the very advanced stage of the disease. When the function of respiration can scarcely be carried on, the body will not then bear large bleeding; but although in the ultimate stage, the vital powers are greatly depressed by the copious abstraction of blood, yet on the accession of the inflammation this remedy seems to be less a matter of choice than of necessity. Dr. Pitcairn was bled once copiously on the third day, and died on the fourth. Sir John Hayes was thrice bled, to the amount, in the whole, of thirty or forty ounces, on the second day, and died on the fourth morning. In both these cases, it must be manifest to every one who reads the history of them, that the patients died not from exhaustion, but from strangulation. By those who will take the pains to consider inflammation as a process, it will be understood that it may be checked, but cannot be suddenly extinguished by the most active means; time must be allowed for the process to decline, even after the fairest efforts have been made to arrest its progress. Unhappily, this necessary time can rarely be ob-

tained in this disease, in which the activity of the inflammation is expended on a small, but most vital seat; for the glottis once narrowed, may be closed by the local action, even after the general powers are prostrate.

These simple, and, I trust, natural views of the subject, lead to a treatment which must be varied according to the stages of the inflammation: the first, or stage of congestion, is the period for active depletion; on the accession of the disease, in the first twenty-four hours, as soon as the patient feels the slightest uneasiness in the larynx, or a very painful and difficult deglutition, which the apparent state of the fauces cannot explain. There is not in this stage any objection to copious general bleeding, suited to the strength of the patient; but if this treatment fails to relieve, and the difficulty of breathing supervenes, it is too certain that the narrowing of the glottis has commenced. In this second and alarming stage, or that of effusion, it may be proper to try local blood-letting, by free cupping from the neck and shoulders, and by a continued oozing from numerous orifices made by leeches on the throat. In both stages it is reasonable to resort to those internal means which diminish the force of the circulation in the capillary arteries; and to produce this effect, none seem better adapted than the combined powers of the tartarized antimony and the submuriate of mercury, in doses suited to the strength of the patient.

In the advanced period of this second stage, when the glottis is so much narrowed that the signs of strangulation begin to appear, a perseverance in these or other ordinary means seem no longer to afford even a chance of preserving the life of the patient, his physician must now resign him to the horrid death of slow suffocation, or make a last effort to save him by the operation of bronchotomy. That the disease is a local one, that it is acute, and of short duration, that it affects a mucous membrane, which consequently does not readily take on the adhesive inflammation, or if it should be so acutely inflamed, easily sheds the lymph that had been effused on it, are circumstances which, in the otherwise hopeless state of the patient, justify a trial of this operation. But if it be further considered, that bronchotomy has actually preserved life under the more unfavourable disease of cynanche trachealis, the operation becomes expedient. And here I am happy to find that the resort to bronchotomy, in Case II. is supported by the opinion of Dr. Baillie, who had actually proposed this operation in one of his cases, which was only not performed because the surgeon arrived too late. He also enforces this opinion by the following advice. " If no substantial advantage is produced by this plan* in thirty hours, it might

* Viz: At the very beginning of the attack, one copious bleeding until the patient faints, followed by the use of opiates, to remove the spasm, which, in his opinion, has some share in producing the difficulty of breathing.

be advisable to perform the operation of bronchotomy at the upper part of the trachea, just under the thyroid gland. This operation would probably enable the patient to breathe till the inflammation in the larynx, more especially at the aperture of the glottis, had time to subside."

Different parts of the windpipe have been selected for the operation by different surgeons. This seems to me a matter of considerable importance; but to enter upon it would lead me out of my province. It is more within the line of my duty to consider what forms of cynanche admit of the operation. In the cynanche maligna, the inflammatory action, which is vehement, effuses coagulable lymph on the tonsils, and, in the worst cases, on the mucous membrane of the larynx; but the strength of the action is soon spent, the lymph perishes, and the surfaces which effused it die, and slough, or open into ill-conditioned ulcers. In such a disease, if symptoms of strangulation supervene, it is almost needless to say, that bronchotomy is inadmissible. But the badly ulcered or sloughing tonsils, with typhoid fever, sufficiently distinguish this malignant disease from the simple cases which demand the operation. In every uncombined case of cynanche laryngea, when other means have failed, I think that the patient ought to have the chance, even if it should prove a small one, which this operation affords. Every case of simple cynanche trachealis is so nearly allied, both in its nature and manner of

proving fatal, to the cynanche laryngea, that the same mode of treatment, with a little modification, which is applicable to the one, is also suited to the other. But when the cynanche trachealis is combined with pneumonic inflammation, of whatever kind, then the operation cannot avail. In proportion as the inflammation is extensive, it is less intense in the larynx, and I think it is not difficult to discriminate between the cases, at the period when the operation is required, by the manner in which the respiration is conducted. In the one, the difficulty of breathing is evidently that of impending strangulation; in the other, that of deep seated and extensive oppression. The difference may, perhaps, be illustrated better by a case of each kind.

Case of Cynanche Trachealis.

—— Williams, a fine boy, at the age of two years and five months, was carried off by an acute disease in little more than forty-eight hours after the attack. The distressing and violent symptoms which marked its progress, and the rapidity with which it proved fatal, induced his mother to solicit me to ascertain, by dissection, the cause of his death. Previously to the examination, I minutely enquired into the circumstances of the case, and the following simple, but impressive history, is given nearly in her own words.

On the 8th of July, 1806, her attention was directed to the boy by his unusual hoarseness; his voice was gruff. He had also a peculiar cough, which a lodger in the house noticed, and said, it distressed him to hear it. But she was less on her guard, because the boy played as usual, and was quite cheerful: she applied, however, to a druggist, and purchased a mixture for his cough. On the following day his skin was hot; he was thirsty; his urine was high coloured, and, on standing, copiously deposited a white sediment; his bowels were regular. His appetite was not yet impaired; at dinner he eat rather greedily a slice of pork, and some cabbage, and afterwards a great deal of fried meat; but at almost every mouthful, he said, water, mamma, more water. As the evening advanced, her anxiety on his account increased, and a nurse advised her to apply a blister to the upper part of the chest. Having complied with this advice, she sent for an apothecary, who told her that the child was in danger; that the blister was as good a remedy as she could use, and did not prescribe any thing else. The boy was yet playful, and at nine o'clock, whilst she held him in her arms, he remarked a kite in the air, and pointed it out to her. His breathing was then shrill and difficult. All the ensuing night his skin was very hot; he took no more food, but called for water continually. A noise attended every respiration, which she ex-

pressed by the monosyllables—flip, flap. It seemed to her that something was lifted up every time he breathed, and in striving to breathe, his head, body, and limbs worked*. On the 10th all the symptoms were aggravated, the respiration became every moment more difficult; he coughed less, and spoke faintly; his lips, neck, hands, and the bottoms of his feet turned livid, his skin became cold, and he seemed to be insensible for an hour before his death, which happened at four in the afternoon.

Examination of the body on the 12th, at six o'clock in the evening, fifty hours after death.

External appearances.—The body was very fat, and seemed to have been vigorous. The feet, hands, and lips were quite livid; the posterior parts of the body much discoloured, but not so entirely livid as the former.

Thorax.—When the sternum was removed, the lungs, instead of collapsing as they usually do, remained turgid with air. On each side of the chest the pleura was free from adhesion, and there was no serous effusion. The cellular membrane of the lungs, opened by sections of different parts, was dry, except where blood-vessels or ramifica-

* This simple description of the mother sufficiently marks the continued muscular efforts, partly involuntary and convulsive, which were made to prolong respiration.

tions of the bronchia were dissevered: from the latter a whitish fluid issued. The pericardium contained very little liquor.

Throat—The larynx, trachea, and larger branches of the bronchia were removed. On looking into the larynx, through the glottis, the passage seemed to be closed. The *external* mucous membrane of the larynx was not affected; but on cutting open the windpipe posteriorly from the rima glottidis to the subdivisions of the bronchia, a thick layer of coagulated lymph was found on the *internal* mucous membrane of the larynx, extending from the extremity of the epiglottis into the trachea, about a finger's breadth below the cricoid cartilage. The rest of the membrane lining the trachea and bronchia was visible, and of a red colour. The sacculi laryngis were completely concealed, the greatest deposition of lymph having taken place near this portion of the windpipe, by which it was so much narrowed, that there was scarcely room for the point of a crow's quill to enter. It also contained a puriform fluid, some of which lodged in the narrow passage within the coagulated lymph, gave the impervious appearance before noticed, and probably occasioned, by its ascent and descent during respiration, the peculiar noise which the mother described.

In the head and abdomen the appearances were

those which belong to the most perfect state of the several organs.

This was a case of simple croup, in which the life of the child might probably have been saved by the timely performance of the operation of bronchotomy*.

* This operation having been performed frequently and successfully in cases of foreign bodies lodged in the windpipe, it may be affirmed that, under the circumstances which demand its employment, whilst it constitutes the only means of preserving the patient, it is in itself, the mere operation being considered, nearly void of hazard. It has not been so frequently performed in cases of Cynanche Trachealis. On this account I may be allowed to extract from the inaugural Dissertation of Dr. Thomas White, published at Leyden in 1786, a most happy case of this kind. "Puer quinque annorum per duos vel tres dies signis Cynanches Trachealis laboraverat, cum medicus advocatus fuit. Ægrum invenit anhelum, fere strangulatum. Respiratio enim maxime fuit laboriosa, atque spiritûs emissio adeo impedita, ut vix flammâ facis speculove prope os admotis sentiri posset. Die Februarii undecimo, Anno 1782, rebus ita se habentibus, emeticum hor. X. A. M. præscriptum fuit, atque omni horâ repeti jussum. Quot emetica devoravit, quidve vomitione ejectum, non didici; sed omnibus frustra usus est.

Hor. ix. vespertinâ nil remittente morbo, morteque ipsa minitante, complures chirurgi tracheam incidendam esse consentiebant; quod statim perfecit Domin. Andree. Incisione factâ, aer magna vi prorumpebat, respiratio protinus facilius absolvebatur, atque æger (cujus vox ante vix audiri potuerat) exclamabat, se nunc levatum, se nunc sanum esse. Tussis vehementissima per canulam argenteam, aut tubum cavum flexilem, (qui Gallicè Bougie vocatur) intra plagæ oras immissum excitata est, nec prius cessavit quam ea penitus fuerunt amota. Dein vulnus pannо ex nebulâ lineâ confecto tegebatur.

Die

Case of Cynanche Trachealis, combined with Pneumonia.

Miss S. three years and a half old, had been for three days affected with a catarrh and hoarse cough. Her mother, during this period, had

Die Feb. xiimo. delirium leve noctu supervenit. Spiritum nunc facilius ducit. Calor et pulsuum crebritas minuuntur.

Die Feb. xiiimo. Nocte magna copia muci flavi pus referentis (qui a bronchiis plane excreari videbatur) e vulnere effusa est, totumque thoracem externè humectavit. Hodie facies, et præsertim musculi oris, levibus convulsionibus afficiuntur.

Die Feb. xivmo. Pyrexia multo minor, facilis per os spiratio, vox quæ ad hoc usque tempus submissa ac stridula fuit, nunc ad tonum naturalem redire incipit. Mucus minus copiosè ejicitur.

Die xvmo. Omnia symptomata leviora.

Posthac nil dignum notatu occurrit; intra mensis unius spatium vulnus perfectè fuit curatum, ægerque ad pristinam sanitatem restitutus.

Duobus abhinc annis valetudo permansit bona, nec ullum incommodum ab incisione expertus est puer."

The above interesting report seems to be deficient only in the particulars of the operation; and these, through the kindness of Mr. Astley Cooper, I have obtained. They are given in the words of Mr. Andree. "At nine in the evening, in the presence of several of the faculty, among whom was my esteemed friend Mr. Ford, surgeon, I performed the operation. The child being laid on his back, and kept as quiet as his great restlessness in struggling for breath would admit of, I commenced the operation by a straight incision from the thyroid gland towards the sternum, about one inch and a half in length. I then cautiously laid bare the anterior part of the trachea, without any material hemorrhage or difficulty, except what arose from the child's struggles. I then made a small transverse incision, or rather puncture through the membrane, which connects the second

twice given her a tea-spoonful of antimonial wine, which had freely vomited her. The last dose was given on Friday evening, July the 7th, when Mrs. S. also applied a large blister to her breast. On the morning of the 8th the symptoms were much aggravated; she then gave her an infusion of senna, and at noon sent for Mr. Newington, who considered the disease to be croup, and prescribed one grain of tartarized antimony in one ounce of an infusion of senna, of which a third part was to be given every half hour, till it excited vomiting. He also ordered that two leeches should be applied to one of her arms. Meeting with Mr. Newington in the afternoon, he asked me to see the child. The respiration was performed with considerable difficulty, but it was not noisy, nor was the cough so ringing as I have heard it in cases of Cynanche Trachealis. The character of a general oppression of the respiration,

and third annular cartilages, and then a similar incision between the fourth and fifth cartilaginous rings; the respiration was immediately relieved. I now proceeded, according to the rules laid down by the most approved authors, and as myself had taught in anatomical lectures, to introduce a silver canula into the trachea through one of the orifices. It instantly produced a most violent and incessant cough, until the instrument was withdrawn. A hollow bougie being introduced, produced the same effect. I then contented myself with dressing the wound superficially, cutting an orifice in the plasters to correspond with the openings into the trachea, and covering the wound with a piece of gauze laid loosely on the neck".

yet passive state of the child, as opposed to the convulsive and struggling efforts with which it is performed under a more complete obstruction of the upper part of the windpipe, in cases of simple croup, was striking. The child being of a vigorous habit, and the inflammation acute, it was proposed to draw six ounces of blood between the shoulders, by cupping. I did not visit this child again, but Mr. Newington communicated to me the following particulars: She was cupped, and in the evening a double quantity of the mixture, ordered in the morning, was repeated at four doses, without producing any other effect than great languor and cold perspirations. The breathing continued to be very laborious, but the cough was not so frequent. On Sunday morning, the 9th, before Mr. Newington saw her, her mother had given her a teaspoonful of antimonial wine; she vomited a little, and in the course of the morning had three evacuations from the bowels. In the afternoon the antimonial wine was given in the dose of a teaspoonful and a half without exciting vomiting. The countenance, which was at first very florid, had now become pale and languid; her pulse had lost its fulness and hardness. At seven in the evening he found her a little easier, but very languid. Her mother had administered, without his order, a tobacco glyster; about two scruples of the leaf had been infused for that purpose. On Monday her pulse was weak, and about 160, her breathing

very difficult, her head thrown back, her face pallid. She was perfectly sensible, and swallowed some milk in a hurry, as if in danger of being strangled. A squill mixture was given, and brought up some viscid mucus. In the evening the frequency of pulse and general languor had increased, her lips were slightly livid, but the rest of the countenance was pale, and about twelve o'clock at night she died.

July 12, 1809, the body was examined by Mr. Astley Cooper, in the presence of Mr. Newington and myself, and I noted the following particulars.

External appearances.—The general aspect was pallid. The growth proportioned to the age, and the abundance of fat proved the previous vigour of the child, and that she had been cut off by a very acute disease.

Thorax.—The right cavity of the chest being laid open, a dark coloured fluid appeared in considerable quantity. It had not the character of an effusion from an inflamed surface. A more particular examination ascertained that the œsophagus had been acted upon by the gastric fluid, which had also destroyed the pleura posteriorly, and thereby admitted of an exudation of blood

from the lung, which, together with some of the contents of the stomach, and probably some prior serous effusion from the pleura, made up the quantity of fluid. The left cavity contained a little serous effusion. A partial exudation of coagulable lymph, on some parts of the surface of the pleura, had made it somewhat rough, impairing that polished surface which is natural to the healthy membrane. The pericardium and heart were not affected. The tongue, pharynx, œsophagus, larynx, trachea, and lungs were removed together. On cutting open the windpipe posteriorly through its whole length, its membrane was found besmeared with a brownish mucus, under which it was every where covered with a thin coat of coagulated lymph, that extended into the ramifications of the bronchia, (see plate VII.) but was no where in sufficient quantity to close the pipe: even in the larynx the sacculi were apparent. The lungs had suffered excessive congestion; their parenchyma was very generally red and tumid, yet they floated when thrown into water. The effusion, therefore, into their reticular texture was serous, but the air-cells and minute ramifications of the bronchia were loaded with a yellowish white fluid, the secretion of an inflamed mucous membrane.

Abdomen.—The stomach had been extensively acted upon by the gastric fluid, and in many places

was in a state of dissolution. The other viscera in this cavity were not minutely examined—their general appearance was healthy.

The head was not opened.

This was a case of croup, combined with extensive catarrhal and pneumonic inflammation, in which the operation of bronchotomy was incapable of benefiting the patient.

SOME

REMARKS

ON THE

USE OF NITRAT OF SILVER,

FOR THE DETECTION OF

MINUTE PORTIONS OF ARSENIC.

By ALEX. MARCET, M.D. F.R.S.

ONE OF THE PHYSICIANS TO GUY'S HOSPITAL.

Read December 22, 1812.

IN the interesting account of the poisonous effects of arsenic, presented to the Society by Dr. Roget, and published in the second volume of the Medico-Chirurgical Transactions*, the author has recommended, for the detection of this poison, a test which I pointed out to him, and which, from a variety of experiments which we tried together, with a view to ascertain its comparative merits, we were induced to consider as the most effectual of

* I take this opportunity of stating, at Dr. Roget's request, that the patient, whose case he there related, completely recovered her health, and has remained well ever since.

all the tests hitherto used for that purpose. The method consists simply in adding in succession, to the fluid suspected to contain arsenic, minute quantities of solutions of ammonia and of nitrat of silver; by which means, if the smallest quantity of arsenic be present, a dense yellow precipitate will be produced.

All the particulars respecting this mode of detection having been fully stated by Dr. Roget, with such references to former writers on the subject as the case required, it would be quite superfluous to enter into any further detail on this head. My object in resuming the subject, the practical importance of which need not be pointed out, is to communicate to the Society the result of an inquiry which I have made on the nature of the yellow precipitate, the appearance of which is assumed as denoting the presence of arsenic, and to answer some objections which have been made against this test by Mr. Sylvester, of Derby, in a paper on metallic poisons, recently published in Nicholson's Journal*.

The yellow compound in question has the following properties:

If, after being well washed with distilled water,

* Nicholson's Journal for December, 1812. Vol. xxxiii. p. 306.

it be suffered to stand for some time in an open vessel, it gradually passes to a brown colour; but it does not, like nitrat of silver, become black on continuing this exposure.

It is readily soluble in dilute nitric acid. It also dissolves on adding an excess of ammonia at the moment of its formation; but after it has been separated and dried, it is no longer sensibly soluble in ammonia.

If a small quantity of this precipitate be exposed to the heat of a lamp on a slip of laminated platina, a white smoke arises from it, and metallic silver remains attached to the platina. The reduction of the silver, in the form of a globule, is still more distinct and striking, if a little carbonaceous matter be mixed with the precipitate, and the blowpipe applied.

When the yellow precipitate, inclosed in a tube, is exposed to the heat of a lamp, the white smoke condenses on the cold part of the tube, in minute octohedral crystals of arsenious acid.

It appears, therefore, that the precipitate in question is a combination of white arsenic (arsenious acid) and silver, or an arsenite of silver; and it is inferred that its formation, when ammonia and nitrat of silver are added to a mixture containing arsenious acid, is owing to a double elective decomposition of the arsenite of ammonia, by the

nitrat of silver, in consequence of which arsenite of silver is formed, and separates as an insoluble precipitate from the nitrat of ammonia which remains in the solution. The addition of ammonia is necessary, because arsenic acid alone cannot decompose nitrat of silver; but in Fowler's solution, in which the arsenic is already combined with an alkali, the decomposition takes place at once, without any addition of ammonia. The fixed alkalies, therefore, can answer a similar purpose; but ammonia has this advantage, that it does not, when added singly, decompose nitrat of silver, a circumstance which, in using the fixed alkalies, might occasion some confusion*.

With regard to Mr. Sylvester's objection, I shall, previous to my offering any remarks upon it, state it in his own words. "If ever muriatic acid be present," says this gentleman, "the test is then wholly useless, as a muriat of silver will be immediately formed, and the yellow compound, said to

* It is necessary, as Dr. Roget has observed in the paper already quoted, that the quantity of ammonia should not be too large; for in that case the precipitate is redissolved. But, even then, it may be made to reappear, by the addition of nitric acid in sufficient quantity to saturate the alkali. In this case, however, the precipitate is not permanent, owing, I find, to its being soluble in the nitrat of ammonia which is formed in the process. Carbonat of ammonia has also the property of producing and redissolving the precipitate.

The fixed alkalies in excess have not the power of redissolving the precipitate.

be so unequivocal in its indication of arsenic, of course be prevented from appearing."

This danger of ambiguity, however, though applying in some degree to the process in question, and well deserving to be noticed, will be found to have been greatly overrated; and there are such easy and obvious means by which this ambiguity can be entirely removed, that it can make no solid objection to the utility of the test.

There cannot be the least doubt, as Mr. S. observes, but that whenever nitrat of silver is added to a solution containing muriatic acid, a precipitate of muriat of silver must be the consequence. But if the nitrat of silver be added in excess, the arsenite of silver is also thrown down by the intervention of ammonia, and a mixed precipitate of luna cornea and arsenite of silver is obtained, which partakes more or less of the yellow colour of the latter, according to the proportion of the two salts.

If to this dubious precipitate a few drops of dilute nitric acid be added, the arsenite of silver is instantly dissolved, and the muriat of silver, which is insoluble, immediately resumes its peculiar density and whiteness. If a little ammonia be now added to the clear fluid, the yellow precipitate appears in the most distinct manner, and becomes even more characteristic from a comparison with

the white precipitate, the appearance of which differs from this in every respect.

By this method, I believe that every objection to the test will be removed; and in order to anticipate all ambiguity, and to avoid any complication or practical difficulty in its application, I would propose to modify the process in the following manner:

To the suspected fluid, previously filtered, add, first, a little dilute nitric acid, and, afterwards, nitrat of silver, till it shall cease to produce any precipitate. The muriatic acid being thus removed, whilst the arsenious acid (if any, and in whatever state) remains in the fluid, the addition of ammonia will instantly produce the yellow precipitate in its characteristic form. It is hardly necessary to add, that the quantity of ammonia must be sufficient to saturate any excess of nitric acid which the solution may contain.

HISTORY OF

A CASE

OF

REMITTING OPHTHALMIA,

AND ITS

SUCCESSFUL TREATMENT BY OPIUM.

By JAMES CURRY, M.D. F.A.S. &c.
AND SENIOR PHYSICIAN TO GUY'S HOSPITAL.

Read Nov. 24, 1812.

IN the earlier period of life, my eyes were remarkably strong; and the power of vision very complete, both as to distant and to minute objects. The first injury they sustained, was during my stay at Edinburgh, and that owing to the practice of reading to very late hours in the night, without using the protection of a shade to keep off the direct light of the candles, or a screen to intercept the constant glare of a blazing coal-fire. Still, the only effect of this, was simply fatigue of the eyes; from which they would probably have recovered entirely, by mere rest. But having imprudently ex-

posed myself upon one occasion, by getting out of bed, and standing for some minutes subjected to the night air at an open window, in consequence of a quarrel in the street, I was attacked, in a day or two after, with acute ophthalmia in the left eye; which, however, was in a great measure removed, in about a week, by the application of leeches, and the use of saline purgatives. A slight degree of obscure vision remained, from turgescence of the vessels in the fore part of the cornea; but this gradually lessened, so as, in a year after, to be scarcely perceptible to myself, and not at all to the examination of another. A subsequent residence of eight months in Bengal, during which I suffered no less than five attacks of severe illness, in the different endemic forms of intermitting and remitting fevers, &c. not only radically impaired my general constitution, but laid the foundation of that particularly weakened state of eyes, which I have ever since laboured under; every *general* febrile attack at that time, being followed by a recurrence of ophthalmia: and even a slight relapse of intermittent, which I sustained during the short prevalence of a westerly wind immediately after doubling the Cape of Good Hope on the voyage home, was attended by the same sequela*. From the period of my re-

* The chief mate of the ship had also a relapse of ague, but in both it was slight, and stopped after the second paroxysm by a remedy much used by the natives of India, for the cure of recent intermitting fevers.

turn in September, 1787, my eyes gradually gained strength; and would probably have continued to do so, had it not been for an unlucky accident. During the very severe weather in January, 1789, at which time the Thames was so completely frozen as to have booths erected upon it, a heavy fall of snow was immediately succeeded by a rapid thaw; and the pipes belonging to the house in which I resided, being all choked with ice, the snow water in the leaden gutters of the roof, rose so high as to get under the tiles, and made its way down through two floors, so as to come into the bed where I lay. How long I was exposed to its influence in the slow mode in which it descended, is impossible exactly to say; but when I awoke through the cold, I found the left side of my night cap, and my left shoulder and arm, with the bed clothes covering them, quite wet. After putting on a dry shirt, shifting the bed clothes, turning the feather bed, and drawing the bedstead into another part of the room, I went to sleep again; and hoped that no inconvenience would ensue, as I had more than once been similarly wetted in my cot by sea-water, without any bad consequence. In a few days, however, I was attacked with ophthalmia in my left eye, which proved more severe and obstinate than any preceding attack; confining me three weeks to the house, and nearly a week of that time to bed: even when the continued pain was abated by leeches to the cheek and temple, followed by a blister, and aided by purging, the vessels of the eye continued

distended with blood, and violent pains, affecting not only the fore part of the eye, but extending all round the orbit, came on every night. Under these distressing symptoms, I consulted the late Mr. Hunter, who recommended the use of the cinchona rubra infused in lime water, which was attended by such speedy relief, that the very first night after I began its use, I was quite free from my usual paroxysm of pain, and the eye instead of being dry and burning hot as before, now discharged tears most copiously, which felt as cold as water from a spring. By continuing this remedy for several weeks, I was enabled to go into the country and settle in business; but I was for many months distressed with inflammation of the glandulæ Meibomii, the acrid secretion from which kept the tunica adnata in a constant state of irritation, and no doubt served to fix still more obstinately, a distinct speck which had formed exactly in the middle of the cornea, so as greatly to obscure the vision with that eye for many years after.

From the period of this attack in 1789, I continued tolerably free from ophthalmia until the summer of 1794, when I was visited by it in a still more formidable shape, about a fortnight after recovering from a smart fit of regular gout; for the pain now was not confined to the fore part of the eye, but extended into the interior, and at times darted through to the back part of the head, with such violence as to resemble what I conceive would

be the effect of a pistol shot. The retina became so exquisitely sensible, that I could not bear the smallest ray of light; and I could discern every article of furniture in a room so completely darkened that other persons were obliged to grope their way, in order to avoid the table and chairs. Being then resident in a country town (Kettering) none of the medical men would undertake to open the temporal artery, which I was urgent to have done, as I knew that under such agonizing pain, I could not long retain my senses. I therefore dispatched a messenger to Dr. Kerr of Northampton, who was luckily met on the road, and very kindly hastened to assist me. His opinion exactly coinciding with my own, he immediately performed the operation, and took away eight ounces of blood before I became faint, but even before half the quantity was drawn, I felt my whole head relieved as from the pressure of a vice, and the darting pains entirely subsided. From that moment, I began gradually to recover, in every respect; but the sight of that eye still remained obscure, from the former opacity of the cornea; and both eyes were at times liable to slight degrees of redness and tenderness from exposure to cold.

In the spring of 1798, I underwent great fatigue and anxiety, in attendance upon a gentleman for whom I had the highest regard; being three days and nights, without taking off my clothes, or lying down in bed more than an hour or two at a time. On the very evening when I was liberated from this

exertion by the patient's death, I was seized, in a few hours after getting home, with increased dimness of sight in the left eye, followed by violent pain, which came on, with augmented severity, in pangs that occurred just at the moment I was dropping to sleep. Having no other mode of relief at hand, I took a grain of opium with some antimonial powder, by which I at last obtained sleep, and the next morning I felt my eye almost free from pain, but with a particular sensation of tenderness upon the cornea whenever I moved the eyelid over it. As the eye was now capable of bearing the light, I examined it attentively by means of a small looking-glass, and distinctly perceived a pit or hollow in the fore part of the cornea about the sixth of an inch in breadth, nearly circular in form, and exactly corresponding with the place where the speck or opacity was formerly situated, but which now no longer appeared: in short it was evident, that the speck had inflamed, and sloughed off during the course of one night: the eye has ever since remained free from any opacity visible to another person; but the *new formed* tunica adnata has not the same fine polish as in the natural and healthy state, and consequently small objects, such as the letters in a book, viewed with that eye, appear hazy and undefined in their edges, unless I look at them through a narrow chink, so as to deflect the rays of light, and make them pass a little to one side of the scabrous part; in which way they are clear and distinct.

This detail may to some appear tedious and uninteresting; but I thought it might be satisfactory to the Society, as it certainly would be to me, to have a complete history of the rise and progress of a complaint, which subsequently displayed itself in so distinct a shape, and with such severity of symptoms as I shall now describe; being perfectly convinced, that nothing will contribute so much to enlarge our knowledge of pathology in general, as that concatenation and sequence of phænomena, which at present are often considered as distinct disorders, though in fact only variations of one genus of derangement in the human system.

On Friday the 23d of Dec. 1802, after going round the hospital, and subsequently visiting some private patients, I returned home at 3 o'clock in order to dress myself for dining out; but before four o'clock, I felt so much stiffness in my *right* eye, that fearing the consequence of exposure in coming back at night, as well as drinking any wine under such threatening, I sent an apology, and determined upon staying within doors all the evening. By five o'clock the eye was become very stiff, with some degree of soreness; and so dim, that the candles appeared dull, and surrounded by an extensive halo or haze; and at eight the pain was so considerable, that I could not read. I therefore took at going to bed a grain of opium, three grains of calomel, and two of antimonial powder; afterwards drinking a basin of water gruel to promote

perspiration. I passed but an indifferent night, however; and in the morning, finding the eye considerably inflamed, I applied six leeches round the lower and outer part of the orbit, and took some colocynth extract, which operated 2 or 3 times with slight relief. In addition to these measures, I put some of the vinous tincture of opium (the old Tinctura Thebaica of the London Pharmacopœia ed. 1740) into the eye several times, by means of a large camel's-hair pencil; this occasioned very pungent pain for a few seconds, and was followed by a short abatement of pain, which, however, was more comparative than real, for in a quarter of an hour after, the eye felt no better than before. At night the pain again increased, and I repeated the opium &c. with relief, taking at the same time some liquor ammoniæ acetatis to excite diaphoresis. The pain abated in the night, and I got some sleep; but no amendment appeared next morning in the eye, which was now still more red than before. On the third night I blistered the right temple, and repeated the opiate, calomel, and diaphoretic, without gaining any more than a temporary benefit. On the fourth day, I became extremely alarmed by the increasing obscurity of vision, which was now so great that I could not distinguish the bars of the window sash, and could barely count my fingers when held within a foot of my face; added to which, this was now not only in the *right* eye which had never been more than slightly affected before, but a similar though less

obscurity, was making progress in the *left* eye also. I therefore sent immediately for an eminent surgeon-oculist, whom I will beg leave on this occasion to designate by the name of *my friend*, a name to which his kindness and attention give him a just claim. He came to me soon after, and upon examining the eyes, remarked, that there was no opacity in the cornea; a circumstance which I had noticed myself before his arrival as different from every former attack; though from the dimness of my vision, I could not ascertain this so well as he did: I cannot describe this peculiar state of vision better than by saying, that every object which I could discern, appeared of a dusky grey colour as if obscured by smoke or steam; and such as I conceive would be the case, if the aqueous humour of the eye had lost its limpid transparency, and acquired the blueish-grey tint, which I have noticed in some mineral waters. I held this as probably indicating an opacity of the capsule of the crystalline lens, or of the vitreous humour; and I could perceive from my friend's manner, that he had much doubt about my recovery. I then stated to him the steps that I had already taken, all of which he approved of, excepting the opium; advising me by all means to abstain from it, as it would tend to weaken the vessels of the eyes, and certainly *retard*, if not, in the end, *entirely prevent*, them from regaining their natural strength. However reluctant I felt, at giving up the employment of a medicine from which I had found at least temporary relief, yet consider-

ing the disease as now altogether beyond my own management, and having the utmost reliance upon his judgment founded on long experience and extensive observation, I determined to submit to whatever measures he might think proper. As the most ready means of lessening the extremely turgid state of the vessels on the tunica adnata, he proposed scarification; which was immediately performed, by drawing down the lower eyelid, and scoring its surface in several places with the shoulder of a lancet, so as to take away about a teaspoonful of blood. No sensible relief attended this operation; on the contrary, the clots of blood which formed upon the incisions during the night, acted like so many foreign bodies, and aggravated the pain whenever the eye-lid moved upon the ball. In addition to this, he recommended fomenting the eye with a decoction of poppy-heads, which was accordingly employed, as soon as it could be got ready. I had always found ease before from cold applications; and the present trial of an opposite temperature, concurred to strengthen my former experience; for the pain became so much worse under the fomentation, that I could not continue its use above a few seconds. Observing no abatement of inflammation when he came next day, he conceived that the failure depended upon the quantity of blood evacuated, being too small to lessen the congestion: and as a more adequate depletion, opened the vein which runs between the inner canthus of the eye, and the nose, and

drew away a large tea-cupfull of blood (about six ounces). During this operation, I thought my head felt lighter, and the eye less stiff and full; but the relief did not last much beyond the flowing of the blood; and the pain at night, with the redness and turgescence of the vessels next morning, were exactly the same as before. My pulse being now languid as to strength, and unaccompanied either by hardness in its stroke, or by febrile heat of skin, he did not consider farther loss of blood, either generally or topically, at all adviseable; but recommended a large blister to the head. I accordingly had my head shaved without delay, and a blistering plaster applied over the whole of the scalp, extending from one ear to another, and from the top of the forehead to the nape of the neck. Notwithstanding what might have been expected from the irritation of so extensive a surface, I certainly slept better than I had done for the three preceding nights; but this was all the advantage it procured, as the inflammation was not in the least diminished by it; and the succeeding night was attended with the same severity of pain as before. I now, agreeably to his desire, took a quarter of a grain of the muriate of quicksilver, dissolved in half an ounce of the spirit of nutmeg, and diluted with half a pint of warm water-gruel sweetened to the taste. This excited most profuse sweating for several hours, and gave me greater ease than any thing which had previously been done: indeed I had observed in former and slighter attacks, that sweating always procured

relief from pain, especially while the diaphoresis was present. I accordingly repeated the same medicine on the following night with very sanguine expectations but by no means with equal success, the pain being more severe although the sweating was nearly the same. The third night it failed entirely; and owing to the nutmeg spirit then employed being made by uniting the essential oil with proof spirit by triturating it with sugar, instead of distilling them together, the muriate of mercury was probably not completely dissolved, in consequence of which it occasioned considerable sickness and vomiting: observing, however, that as often as I strained to vomit, though nothing was brought up except the gruel, the pain of the eye was sensibly lessened for a short period, I conceived that fuller vomiting might be attended with still greater and more permanent benefit, and with this view got to a medicine chest which stood in the room, and guessing five grains of tartarised antimony by lifting it between my finger and thumb, I placed this in the palm of my hand, and by wetting the point of my finger, took up what I supposed might be from half a grain to a grain: I swallowed this quantity every quarter of an hour, until I became very sick, and vomited several times: after which I got ease and fell asleep. Next day, the eye appeared exactly as before; and feeling great debility, and loss of appetite, I did not recur either to the sweating or the vomiting plan. The nocturnal exacerbation of pain having now become

greatly aggravated, as well as more distinctly marked in its remitting form, I had recourse to the cinchona, from which I had formerly experienced such remarkable advantage; but although taken in the same way, and in doses of a drachm every four hours, it produced no sensible effect. One article still remained, which is known to possess extraordinary powers in the cure of intermitting fever, even where the Peruvian bark has failed; I mean the arsenical solution. This, therefore, I had recourse to, in doses of ten drops three times a day; but after two days' trial, it occasioned such loss of appetite and of general strength, without any perceptible advantage to counterbalance these effects, that I abandoned it as I had done the rest. I was now left, in the middle of the third week, apparently without any resource; my sight being only capable of distinguishing between day and night; the flame of the candles upon the table appearing like two drops of blood, and the fire when burning bright, like a mass of red-hot iron, whilst all around was utter darkness: in addition to which, the paroxysms of pain through the night, could only be compared to screwing the eyeball out of the socket; and the only ease I had from this, was by sitting up in bed, with a wash-hand-basin of cold water placed between my knees, and dipping a towel in this, which I applied to both eyes. Just so long as the sensation of cold lasted (the ground was then covered with snow), so long was the pain rendered bearable; but as soon as the

wet towel acquired a little warmth, the pangs returned as before. Worn out by suffering, and with the melancholy prospect of irremediable blindness before me, I resolved to obtain ease at all events, by having recourse to opium; and wishing to procure this more particularly in the night, when the pain was always worst, I took *two* grains of solid opium at six o'clock in the evening. I can scarcely express the delight I experienced on finding, that instead of the paroxysm coming on between 7 and 8, as usual, I felt quite easy; and on going to bed at ten o'clock, got into a sound sleep from which I awoke greatly refreshed and free from pain, at 8 next morning. The next day also, was comparatively a day of ease; and it will readily be concluded, that I did not fail to take the opium again as on the preceding evening. The second night, like the first, was *a night of bliss*, which was perhaps the more apparent, by my not sleeping at all, but lying *in perfect ease*. On the following morning, my friend called, after an absence of two days (for indeed I believe the obstinacy of my case had put his kind attention to a severe trial by previous daily attendance); and it was with unspeakable pleasure, I heard him pronounce my eyes considerably better. I then candidly stated to him, that the severity of my sufferings had compelled me to disobey his injunctions against taking opium; and that, as I had left off every other medicine except the occasional use of colocynth pill with calomel, as a laxative, I could only attribute the amendment to the effect

of that remedy. Under such circumstances, he admitted, that I should continue its use; observing, however, that there was either something very singular in the nature of the case, or that opium was peculiarly friendly to *my* constitution. Notwithstanding the strong proofs which I already had of its beneficial influence, I was by no means free from fears, that in thus procuring temporary relief, I might not ultimately do injury to my eyes; and believing that I had now in my power the means of effectually checking the farther progress of the disorder, I was determined to maintain my professional allegiance, by abstaining from opium until it became imperiously necessary. Accordingly, on the third evening I did not take my dose as before at six o'clock, but waited the coming on of the paroxysm, and though not without some anxiety, yet certainly with greater hope, that I might either escape the attack altogether, or have it in a very mild and bearable degree. At eight o'clock, I observed the usual precursor, a feeling of soreness, aggravated by pressure, in the integuments round the eye, and especially at the *infra* and *super-orbitary* holes, through which the nerves pass to supply these parts. In twenty minutes the pain became very severe, and I immediately swallowed two grains of opium. At nine o'clock the pain, instead of abating, was evidently upon the increase; I therefore repeated the dose; and having remarked that the full effect of any individual dose of this medicine is generally experienced in three quarters of an

hour from the time of taking it, I determined to repeat the same quantity *every hour* at farthest, until relief ensued; and as no sensible abatement took place from what was previously administered, I persevered at the assigned intervals, until I had swallowed *ten grains* of solid opium, without the smallest alleviation of my sufferings. At twelve o'clock, I became stupid to every sensation, except that of agony; and retired to bed in a state which can better be conceived than described; nor do I exaggerate the description of my wretchedness when I declare, that I should have blessed the friendly hand which presented a pistol to terminate my existence. For *fourteen hours and a half*, I lay in this struggle between stupefaction and torture; the muscles of the right side of the face quivering with pain as if torn from the bone, and a continued stream of scalding tears running down the cheek, whilst the eye-ball itself was, at intervals of only a few seconds, affected with a convulsion that made every limb shake with the agony. At half past eleven o'clock in the forenoon, the power of suffering was itself worn out, and from that time till five in the evening, I lay in a state which alarmed the people in whose house I lodged, as I neither called for nourishment, nor even rang the bell to say I wanted assistance of any kind. My conviction, so dearly purchased, was now complete; and I resolved, whatever might be the ultimate event, never to pass such another night, whilst I had the certainty of relief within my power. Accordingly,

at six in the evening, I took *three* grains of opium, with the same happy effects as at first; and by repeating this every evening at the same hour, I not only completely prevented all recurrence of pain, but in the course of ten days, was able to go out daily in a carriage to visit a patient at Canonbury; and at the end of three weeks, resumed my hospital business and lectures as before my attack.

The case just related, offers grounds for much new and interesting inquiry respecting ophthalmia in general, and particularly that form of it which has committed such ravages among those of our soldiers and seamen who were employed in the expedition to Egypt: as I am strongly inclined to believe, that they bore a considerable resemblance to each other in many essential points. At present, however, I have no leisure for so extensive an investigation; and were I even so disposed, I have not had sufficient opportunities of seeing the Egyptian ophthalmia as it has been prolonged or propagated in this country, to qualify me for such an undertaking.

One circumstance has surprised me greatly, which is, that in the course of a pretty extensive examination of authors, both ancient and modern, which I have been obliged to go through in order to collect materials for my lectures, I have not met any case resembling my own. Does this proceed from its being unique in its kind, or from want of

due discrimination in those who have treated on ophthalmia, especially in its epidemic form? However reluctant to admit the latter conclusion, yet I am compelled to do so when I affirm, that, notwithstanding the almost exclusive treatment of such complaints by the oculist, and the consequent infrequency of their coming under the cognizance of the physician, I have myself met with several of the same kind; and, therefore, reasonably presume, that I am not the only person to whom they have occurred. Being hereditarily of a gouty constitution, and having had to the amount of six or seven severe and regular attacks, beside many slighter and irregular ones, it might be imagined, especially as gouty ophthalmia is mentioned by a few authors, that mine was of that description; and such indeed was the opinion of the friend who attended me; as he acknowledged, that on no other ground could he account for the disorder giving way to opium, while it resisted every mode of treatment which he had before found successful. At first I was disposed to admit this reasoning; but from subsequent attention to the disease, as it has occurred both in myself and in others, I am satisfied that it is not correct: for three of the cases which I have seen besides my own, were in females who are well known to be very rarely the subjects of gout, comparatively with men; and in no gouty attack which I have ever had in the inflammatory form, did I ever receive any considerable relief from opium. The *progress*, likewise, of the two

forms of disorder is very different; for I have had gouty paroxysms of various intermediate duration between ten days and six weeks, whereas the ophthalmia in three distinct attacks posterior to the one related above, has regularly assumed the febrile period of *three weeks*, so markedly so indeed, that in three of a milder degree which occurred in 1806, 1810, and 1811, although by the use of opium I kept the disease so completely in check, as not to be confined from my business a single day, yet it ran its usual period. But a very curious circumstance yet remains to be mentioned, which will, perhaps, of itself be sufficient to disprove the alledged gouty nature of my ophthalmia in 1802-3. It must be remembered by most of the Members of this Society, that the month of January, 1803, was that in which a very violent, and in many instances fatal, catarrhal pneumonia prevailed in London and its environs particularly, but I believe also, though in a less degree, in every part of England. The same disease had previously appeared in some part of Germany, whence it spread to Paris, where it raged with unusual violence under the name of *la gripe;* and it had begun to decline at Paris, about the time it arrived at its greatest height in London. From the date of the memorable influenza of 1782, which was perhaps the most universal ever recorded (having traversed the earth in a westerly direction, from the Tartar hords contiguous to the Great Wall of China, and finally spent itself in the West India Islands and America,)

I had never escaped an epidemic catarrh in its ordinary form, until this time. My escape on this occasion may indeed be accounted for, upon a principle which, though not universal, is yet so general as to be admitted among the laws of the human constitution, viz. that one disease supersedes, or prevents another of an analogous nature, from taking place at the *same time;* and it will readily be granted, that my constitution was sufficiently occupied by one kind of suffering, to entitle me to an immunity from any other as long as the first continued. But will this, as applied to my case, explain why the same freedom from catarrh should equally attend *many other* persons who were affected with ophthalmia about the same time as myself, although in so trifling a degree, as scarcely to confine them to the house? Mr. Stocker, Apothecary to Guy's Hospital, and several of his family, had slight ophthalmia during the prevalence of the influenza; but not one so affected had any attack of catarrh. A person who came to me on business during my convalescence, observing that I wore a shade and green spectacles, told me that he had been himself a sufferer, about three weeks before, with the same complaint in his eyes, as had many of his neighbours, and of his workmen; and upon inquiring whether he or they had also the influenza, he said it had been remarked by all as surprising, that they escaped the epidemic cold. During the summer of 1803, I met Sir James Fellowes, who had lately before returned

from Paris, where he was during the whole time that *la gripe* prevailed; and upon mentioning to him the singular connexion between it and ophthalmia in London, he informed me, that a similar coincidence was noticed in France, with this unaccountable difference however, that *there* the catarrhal pneumonia *preceded* the ophthalmia; but that the immunity was the same in both places, no person who had laboured under *la gripe* being known to suffer afterwards from inflammation of the eyes: Dr. M'Donnell, of Orchard Street, Portman Square, who was likewise in Paris at the period referred to, confirmed by his testimony the information of Sir J. Fellowes, both as to the *order* in which the two forms of complaint took place, and to the exemption which each of them afforded against the other.

The evidence which I have here adduced, will perhaps be deemed by the Society quite sufficient to decide the question in the negative, as to the alledged gouty nature of the ophthalmia; but whatever doubt might remain on that point, it must be entirely done away by the account I shall now give of my last attack, which I watched most minutely through its several stages, with a view to place this circumstance beyond controversy in future.

In the latter end of March 1812, I had a smart fit of gout, the violence of which was greatly mitigated by taking two half bottles of the eau me-

dicinale at the distance of four days, so that I was enabled to go about, though still with inflammation, swelling and pain; and at the end of a month from the commencement, the symptoms spontaneously ceased, leaving me in better general health than I had been for a considerable time. During the latter part of May, though able to walk very well, I was prevented by the coldness and wetness of the weather, from taking any exercise on foot, which is the only kind that is either pleasant or beneficial to me. On Monday the 4th of May, the weather became suddenly much finer than it had been for many weeks, and I determined to avail myself of the change, by dismissing my carriage in Oxford Street about three o'clock, and doing the remainder of my business on foot. The sky was perfectly free from clouds, and the sun considerably hot; but the wind was easterly, and had a bleak chillness in it which made the alternate sensations of heat and cold, accordingly as I was in the sunshine or shade, peculiarly unpleasant. In passing along Fleet Street on my return home, about five o'clock, I felt suddenly a painful sensation in the lower part of my right eye, exactly as if a person had forcibly thrust the end of their finger against the under eye-lid; and as this took place not only without any obvious cause, but as the dull pain continued, though in a less degree, during the remainder of the evening, I had no doubt but it was a prelude to a severe attack of ophthalmia. As soon as I could examine the eye,

I found my alarm was but too well grounded, for there appeared a stream, as it were, of red vessels running from the lower part of the eyeball to the edge of the cornea, exactly at that part where I had felt the pain at first. I lost no time in taking two grains of opium, which procured me a very good night; and as the eye was scarcely at all painful, and had become but very little redder when I looked at it again the next morning, I flattered myself that I should escape without much inconvenience. When the evening came, however, this hope was done away, as the pain recurred, and compelled me to take three grains of opium to get relief. The following morning the eye was very red, and the sight so obscure, that I could scarcely read a letter. As the treatment differed in no respect from that which I have already detailed, it is unnecessary to repeat it here; suffice it to say, that by pursuing the same plan now, I was not kept within doors more than three days, and these not in succession; for after staying at home on Thursday and Friday, I felt so much better on Saturday that I made a number of visits, and in the evening sent to apprize my friend Dr. Babington, who was lecturing for me, that I should resume my hospital duties on the Monday. Very early on Sunday morning, however, the pain came on with great violence, so much so indeed, that it required six grains of opium taken within the first hour, to render it bearable; and eight grains more, at different periods through that day, to keep it off.

In the course of the succeeding week the inflammation abated, and a *second* time did I, at the distance of seven days, announce my intention of returning to lecture; but a *second* time was I disappointed, by the vessels of the eye becoming still more distended, though not more painful than before; and the vision with that eye being as much obscured as in the attack of 1802-3. At the end of the *third* week the pain ceased entirely, and the redness abated so fast, that in four days more I returned to the discharge of my several duties as before my attack.

The complaint, in this instance, differed from that of 1802-3 in two particulars, viz. that the *diurnal* exacerbation of pain was not so great as it was then, whilst the septenary or *hebdomadal* stages were much more distinct; in which respect the first partook more of the intermitting, the latter more of the continued febrile type.

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INDEX

TO

VOLUME THIRD.

A. Page

ALBUMEN of the blood ..... 222.
Anæsthesia, a case of ..... 90.
Animal fluids, on the composition of ..... 198.
Appendix vermiformis, case of diseased ..... 57.
Arsenic, test of ..... 342.
———, note on the case of poison from ..... 342.

B.

Bath, on the douches at ..... 314.
Bell, account of the muscles of the ureters, &c. ..... 171.
Berzelius, general views of the composition of animal fluids .... 198.
Bile, chemical properties of ..... 238.
Bladder, anatomy of the neck of ..... 176.
———, irritable, effects of the muscles of the ureter in ..... 171.
Blane, Sir Gilbert, on the Walcheren fever, &c. ..... 1.
Blood, chemical properties and analysis of ..... 199.
———, general results of its analysis ..... 231.
Bostock, Dr. on diabetes insipidus ..... 107.
———, on the bark of the coccoloba uvifera ..... 146.
Bree, Dr. on splenitis ..... 155.
Bronchotomy, in cynanche trachealis, successful case of ..... 335.

C.

Calculus voided from a tumor in the groin ..... 191.
Cheese, chemical properties of ..... 274.
Chemical properties of animal fluids ..... 198.

Page

Chevalier, case of ovarian dropsy ..... 40.
———, improved method of tying diseased tonsils ..... 80.
———, extravasation within the theca vertebralis ..... 102.
Coccoloba uvifera, experiments on the bark of ..... 146.
Colouring matter of the blood ..... 208.
Copeland, case of calculus voided from a tumor in the groin ... 191.
Corpora carnosa Morgagni ..... 172.
Curry, Dr. on remitting ophthalmia ..... 348.
Cynanche laryngea, cases of ..... 84.
———, further observations on ..... 323.

D.

Diabetes insipidus ..... 107.
Diabetic urine, experiments on extract from .. .. 120.
Difficult parturition from dropsical ovarium ..... 47.
Diseased testicle and viscera ..... 59.
Douches, mode of applying, in Paris ..... 314.
Dropsy of ovarium ..... 40.
———, producing difficult parturition ..... 47.

E.

Earle, case of diseased testicle, &c. ..... 59.
England and Wales, health and population of ..... 34.
Eye, humors of, analysis of the ..... 253.
Excretions, analysis of the ..... 256.
Exercise, its use in sciatica ..... 316.

F.

Farre, Dr. on Cynanche Laryngea ..... 84.
———, appendix to his paper ..... 323.
Fever of Walcheren, &c. ..... 1.
Fibrin of the blood, chemical properties of ..... 200.
Fluids, animal, chemical properties of ..... 198.
Fungus hæmatodes ..... 277.

G.

Gelatine, not present in the blood ..... 233.
Goderneau's powders, analysis of ..... 313.

H.

Page
Health and population of England and Wales ..... 34.

I.

Intermittent fevers ..... 1.
Iron of the blood, inquiry into its influence in producing the colour of the blood ..... 213.

K.

Kino, compared with the coccoloba uvifera ..... 150.

L.

Labour artificially induced ..... 123.
Lactic acid, note on ..... 224.
Langstaff, case of fungus hæmatodes ..... 277.
Lawrence, note on a disease of the testicle and viscera ..... 71.
Lewin, Dr. case of diabetes ..... 114.
Ligature on tonsils ..... 80.
Luette vesicale ..... 172.

M.

Marcet, analysis of a calculus from the groin ..... 197.
———, on a new mode of treating chronic rheumatism ..... 310.
———, on a test of arsenic ..... 342.
M'Cartney, Dr. case of diabetes ..... 117.
Merriman, on difficult parturition from dropsical ovarium ..... 47.
———, on labour artificially induced ..... 123.
Miasmata of marshes ..... 19.
Milk, analysis of ..... 273.
Mucus of the mucous membranes ..... 245.
Muscles of the ureters ..... 171.

N.

Nitrate of silver, with ammonia, as a test for arsenic ..... 342.
Ophthalmia, remitting, case of ..... 348.

O.

Page
Opium, use of, in remitting ophthalmia ..... 348.
Ovarian dropsy, case of ..... 40.
———————, producing difficult parturition ..... 47.

P.

Parkinson, John, case of diseased appendix vermiformis ..... 57.
Parturition, difficult, from dropsical ovarium ..... 47.
Pelvis, distorted, requiring premature labour ..... 123.
Perspiration, analysis of the fluid of ..... 256.
———————— in rheumatism ..... 310, 320.
Philip, Dr. A. P. Wilson, case of disordered respiration .... 290.
Pieromel in bile, not a peculiar substance ..... 238.
Population and health of England and Wales ..... 34.
Presentations, preternatural ..... 137.
Prostate, third lobe of the ..... 172.

R.

Respiration, disordered ..... 290.
Rheumatism, on the remedies in ..... 310.
Roget, Dr. note on the case of recovery from arsenic ..... 342.

S.

Saliva, analysis of ..... 242.
Salts of the blood ..... 222.
Sciatica, new mode of treatment in ..... 310.
Secreted fluids, chemical properties of ..... 234.
Serous membrane, fluid of ..... 251.
Serum of the blood ..... 222.
Silver, nitrate of, with ammonia, as a test for arsenic ..... 342.
Splenitis, case of ..... 155.
Sweating, as a remedy in rheumatism ..... 310.

T.

Testicle, diseased ..... 59.
Theca vertebralis, extravasation within the ..... 102.

Page

Tonsils, improved method of tying diseased ..... 80.
Trigone de la vessie ..... 172.

U.

Ureters, muscles of the ..... 171.
Urine, analysis of ..... 257, 270.
Uvula vesicæ ..... 172.

W.

Walcheren fever, Sir Gilbert Blane on the ..... 1.

Y.

Yelloly, Dr, case of anæsthesia ..... 90.

Explanation of the Plates.

Plate I. illustrates the morbid appearances in the brain, described p. 67.

fig. 1. represents a section of the tumor; shewing the looseness of its connection with the brain, and the bloody and mottled appearance of its structure.

fig. 2. represents the appearance of the tumor in the surface of the brain covered by pia mater.

Plate II. represents the instruments for tying diseased tonsils, described p. 80.

Plate III. was engraved from the drawing of the recent preparation mentioned in the note at p. 87, and represents the morbid appearances in Case II. of cynanche laryngea, described at p. 86.

a. The tongue.

bbbb. The pharynx cut open.

cc. The arch of the soft palate, in the centre of which is seen the uvula of its ordinary size.

dd. The tonsils somewhat tumid, being slightly inflamed.

e. The epiglottis acutely inflamed, and very tumid.

ff. Coagulated lymph which had been effused from the inflamed surfaces about the glottis and epiglottis.

g. The rima glottidis closed by the tumefaction of the mucous membrane, and by the lymph effused on it.

h. The termination of the pharynx in the œsophagus. At this part, and for some space above it, the mucous membrane is not inflamed.

Plates IV, V, and VI. are explained in pages 187, 188, and 189.

Plate VII. represents the morbid appearances in cynanche trachealis, which if contrasted with the morbid appearances in cynanche laryngea, illustrated by plate III, will give a distinct notion of the seat peculiar to each species. The layer of coagulated lymph, represented in plate VII, although thinner, is more extensive than is proper to simple croup, appearing not only on the mucous membrane of the larynx, and trachea, but also on that of the bronchia; for, in this case, the inflammation of the windpipe was combined with inflammation of the lungs: see the case of Miss S. Page 336.

a. The epiglottis.

bbbb. The windpipe cut open posteriorly from the glottis to the subdivisions of the bronchia.

c. Two portions of coagulated lymph raised from the mucous membrane of the trachea, between which and the lymph a black bristle is inserted. From these portions the layer of lymph may be traced upwards to the epiglottis, and downwards into the bronchia. In the larynx its sacculi are apparent, the layer of lymph being there very thin.

This preparation is preserved by Mr. Astley Cooper, who kindly permitted me to have a drawing made from it.

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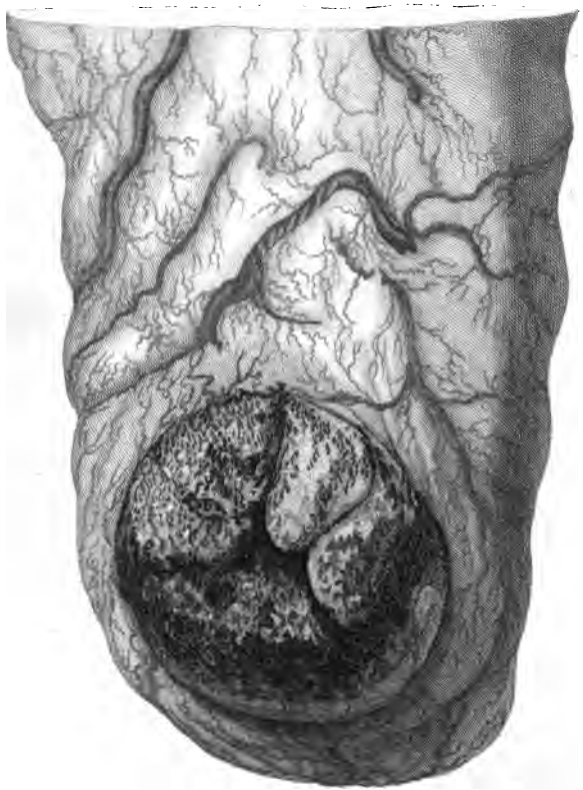
Fig. 1.

Fig. 2.



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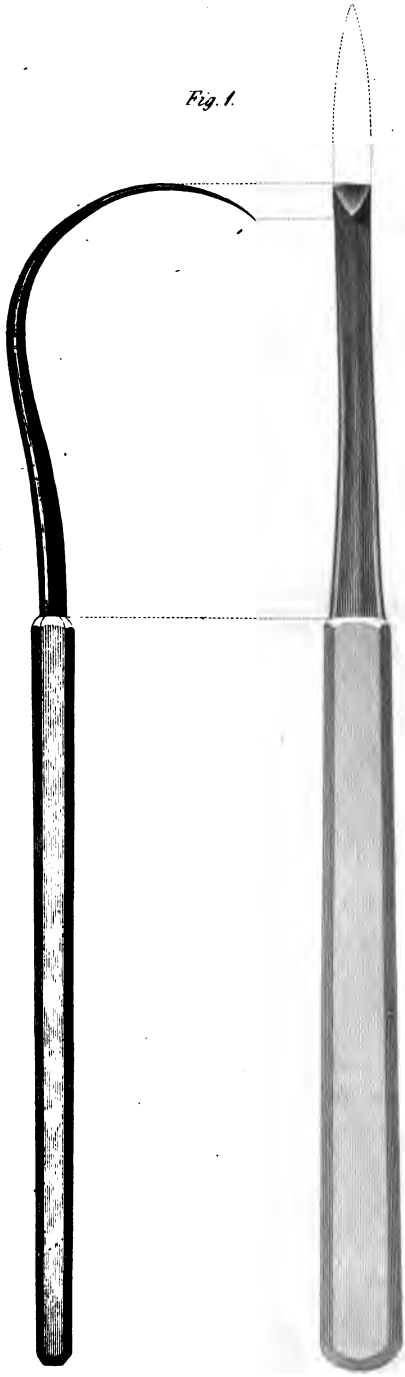

Fig. 2.

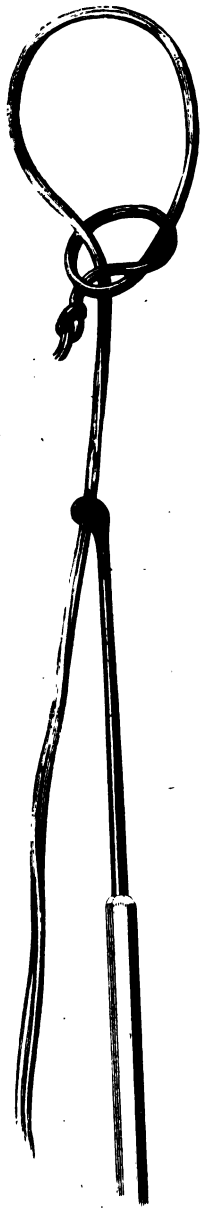

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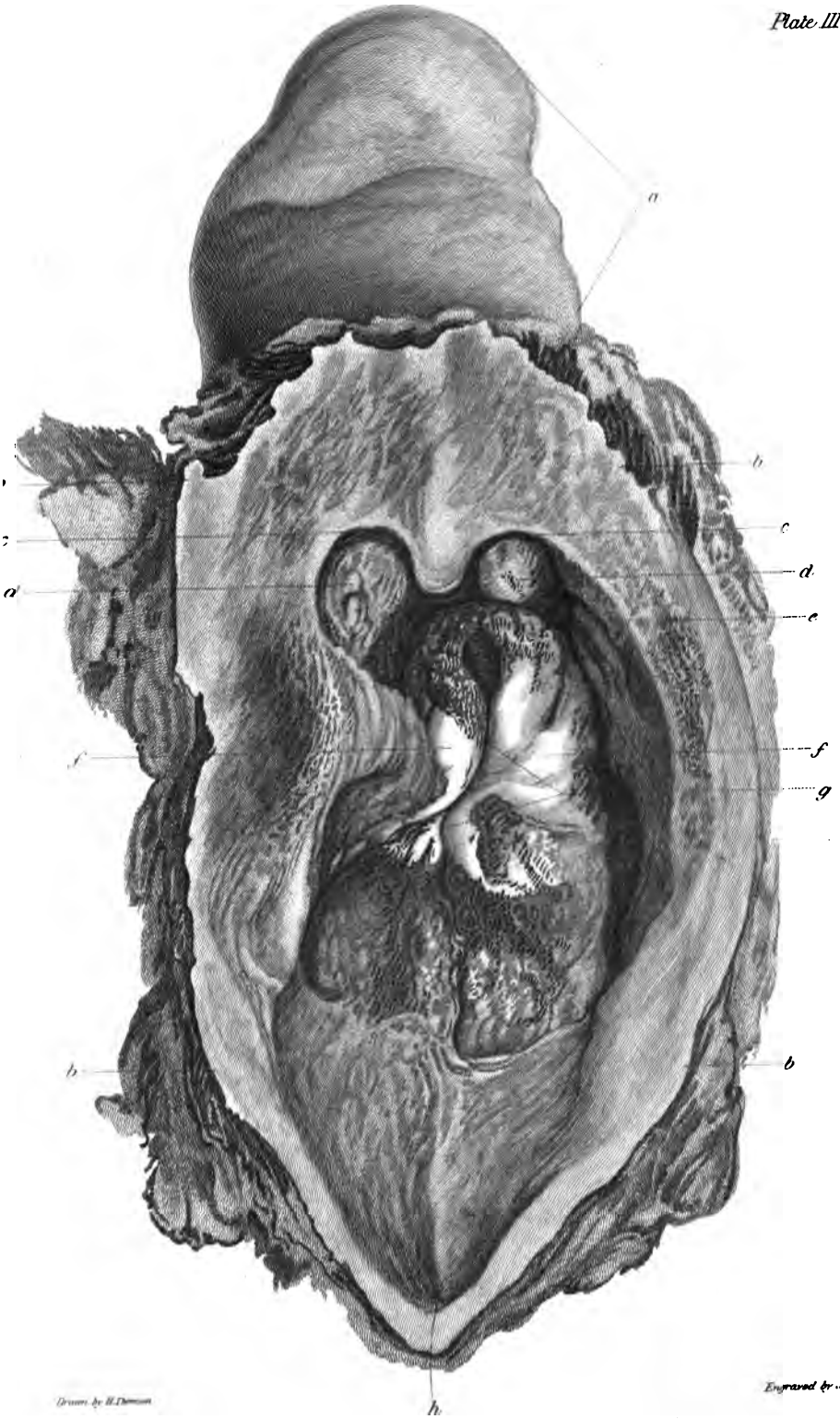
Plate III



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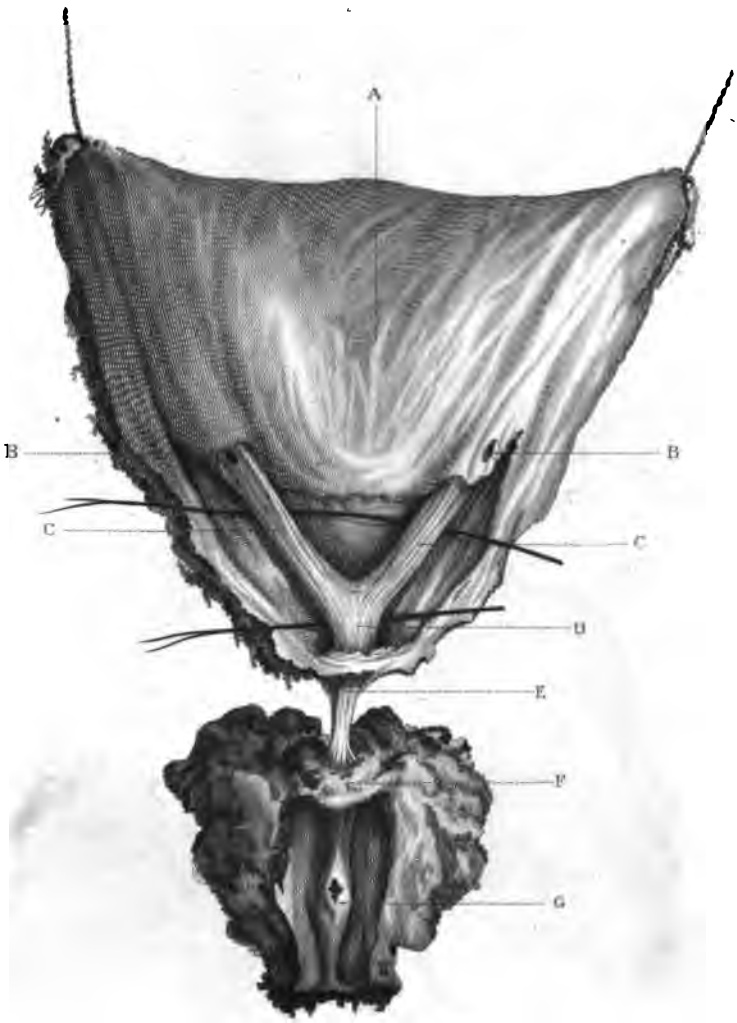

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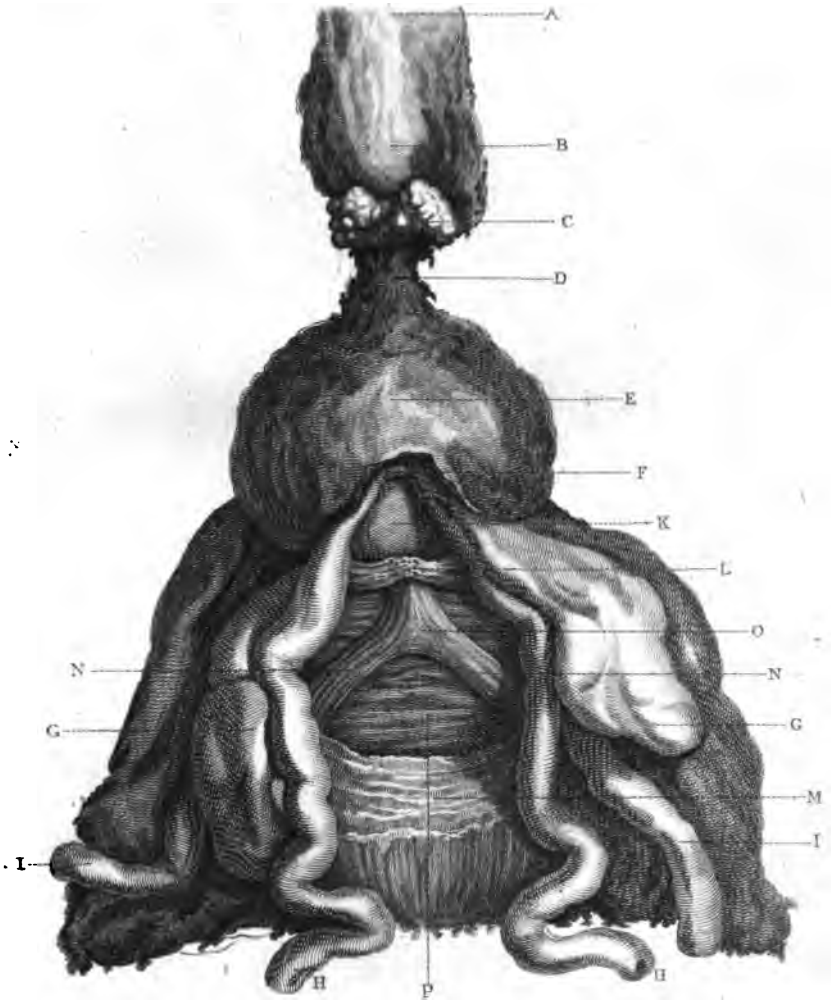

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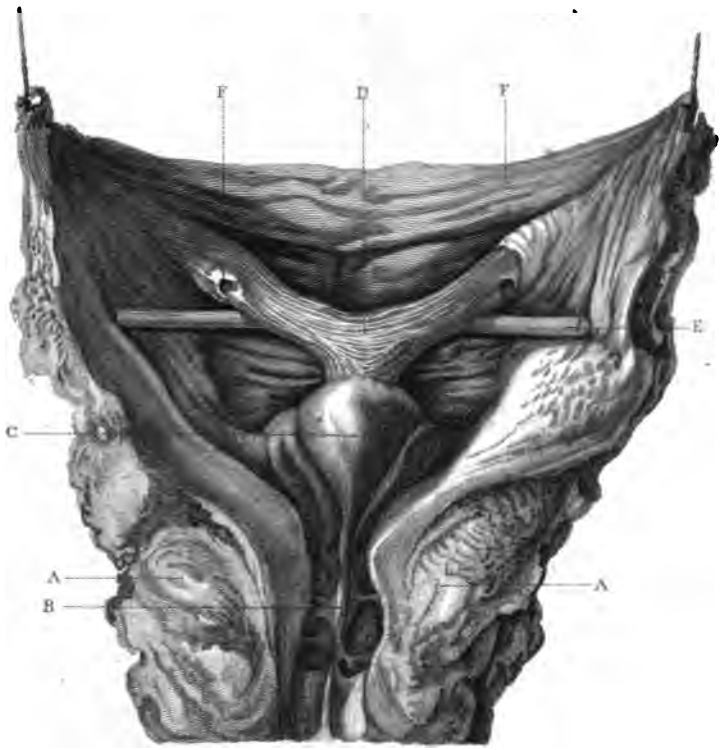

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Engraved by J. Basire.

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